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THE UNITED NATIONS ORGANIZATION

ALTHOUGH the annual reports of the Secretary-General of the United Nations on the work of the Organization have not attracted the attention that corresponding reports on the League of Nations did, and have as yet not gained the reputation enjoyed by those of the Director of the International Labour Organization, they are not unworthy of study. Dr. Trygve Lie's fifth report*, covering the work of the Organization during the period July 1, 1949–June 30, 1950, is no exception. It has something of the realism which characterized the surveys of John Winant and Harold Butler, and the wide range of activities of the Organization are related to its main purpose in a way which never obscures the fundamental conditions on which the success of the United Nations Organization depends.

Dealing first with the detailed activities of the Organization, those of the Atomic Energy Commission perhaps have first claim on the interest of scientific readers. Here the Secretary-General's review takes us no further than the Security Council's decisions of September 16, 1949, and the discussions at the fourth session of the General Assembly. No more need be said here than that the efforts of the Secretary-General to be impartial never disguise the plain fact that the responsibility for deadlock in this matter rests fairly and squarely on the U.S.S.R. and its satellites, through their refusal to consider any effective scheme for international control.

In the section of the report which deals with economic and social questions, the economic advancement of under-developed areas, and particularly the question of technical assistance, are of scientific interest. Such activities expanded considerably during the year, and under the 1950 programme some sixteen countries have requested expert advice in connexion with their plans for economic development, while the services of about fifty experts, representing twenty different nationalities, have been made available to under-developed countries. For example, a group of specialists in agriculture, geology, industry, public administration and other fields began work in Afghanistan in June, and a comprehensive team of experts left for Bolivia in April 1950, to advise the Government on public finance, mining, transport, electric power, labour legislation, social welfare, standards of living, education, agriculture and forestry. A soil conservation expert and an expert on community planning and housing have been appointed in connexion with the Government of India's sponsorship of the Palakarni swamp reclamation and development project. This project is to serve as a pilot scheme, with broad implications for land reclamation and food production elsewhere in India. Arrangements have also been made for the appointment of a resident technical-assistance representative to maintain liaison with the Pakistan authorities and

* United Nations. Annual Report of the Secretary-General on the Work of the Organization, 1 July 1949–30 June 1950. Pp. xiv+143. (Lake Success, N.Y.: United Nations; London: H.M. Stationery Office, 1950.) 11s.

to advise them on the formulation of requests for technical assistance in the light of facilities available through the United Nations. The Secretary-General has established the beginnings of a clearing-house service to handle specific inquiries for technical information and for advice on the location of technical personnel, equipment and supplies. The United Nations Technical Assistance Conference at Lake Success, held during June 12–14, 1950, marked further the culmination of preparations for launching an expanded programme whereby the United Nations and five of its specialized agencies will co-operate to provide technical assistance for the economic development of under-developed countries on a scale far greater than has previously been possible. The agencies in question are the International Labour Organization, the Food and Agriculture Organization, the United Nations Educational, Scientific and Cultural Organization, the International Civil Aviation Organization and the World Health Organization. The plan provides for the establishment of a technical assistance board and a technical assistance committee to review developments in this field and co-ordinate action, as well as to review the working relations between the participating organizations with the view of achieving the fullest possible co-operation. The basis for the expanded programme is now firmly established, and the programme is ready to be put into operation as soon as the funds pledged at the Technical Assistance Conference become available.

The Secretary-General is also undertaking the organization, within the United Nations, of an international centre for training in public administration. The immediate purpose of this institution is to arrange for the training, either abroad or at home, of qualified Civil Servants in specific problems of public administration, so that they may contribute to the improvement of the organization and management of government services in their home countries. The long-term objective is to improve and disseminate the art and science of public administration for the benefit of all nations. The first seminar was to be held at Lake Success during September 15–November 15, 1950, when thirty participants were expected, selected by the Secretary-General from experts which were to be nominated by the governments of member States.

There is a brief reference in the report to the United Nations Scientific Conference on the Conservation and Utilization of Natural Resources, but here, as in all other spheres where the possibility of international co-operation has been so successfully demonstrated, given the will to co-operate and a common purpose, the report reveals how far the world is as yet from any such community of purpose. What is said in the report on such questions as the protection of human rights and the promotion of freedom of information makes this abundantly plain, and in his introduction the Secretary-General shows how real progress in all such technical and scientific or social questions is linked up with the political question of world peace. The major decisions and acts of the United Nations during the year under review, recorded by him as of potential constructive benefit to the world, all turn for realization on a settlement of the conflicts for power and position resulting from the defeat of Germany and Japan.

Dr. Trygve Lie points out frankly that the essential elements of a system of collective security do not yet exist. There is no plan for the control, regulation, reduction or prohibition of armaments, including atomic bombs, nor does any agreement exist regarding the forces that should be pledged by members of the United Nations to enforce decisions of the Security Council against threats to the peace, breaches of the peace and acts of aggression. It is true that, as he notes, the Security Council did act quickly and decisively in Korea; but there is no certainty or probability that it will do so again in the face of the latest and more serious threat, or that it could marshal quickly the necessary forces. While the Secretary-General in his report could take no cognizance of regional defence developments like those under the Atlantic Pact or the Council of Europe, in spite of their evident bearing on this matter, the slow progress made in the organization of North Atlantic defence or a European army emphasizes the difficulties in collective defence even where no major ideological differences are involved.

Much of this introductory part of the Secretary-General's report is concerned with his own efforts and proposals for breaking the deadlock between East and West, and for securing a resumption of negotiations within the United Nations. Nevertheless, it seems clear that he himself leans to that view of the United Nations which regards it as an organization of like-minded nations co-operating to raise standards of life and achievement throughout the world, rather than as a body in which nations of very different outlook may find ways of composing their differences other than by resort to war. The latter theory was, of course, one that for long enlisted support for the League of Nations; but experience showed that in spite of the outstanding success of the League in certain technical fields such as health, opium, etc., the fact of co-operation in those fields had little effect on the will to co-operate in the political field or the composure of differences there.

Even less than when Dr. Trygve Lie wrote in July last is it possible to be confident about the future of the United Nations and world peace; but the widespread support among the peoples of the world for the United Nations and the extent of the constructive work already being carried on under its auspices should nevertheless prevent despair. There must, indeed, be a will to peace before peace can be achieved, and those who wish for peace must be strong and resolute to pursue it. Differences in outlook, in values, in philosophy, between East and West need not prevent a settlement based on mutual respect; but there must at least be a foundation of understanding and of goodwill on both sides. Perhaps the most disappointing feature of the whole five years work of the United Nations is that repeated manifestations of goodwill from the West to the East have failed to bring any evidence of reciprocal goodwill from the East to the West.

A PHYSICIST LOOKS AT WORLD AFFAIRS

Out of my Later Years

By Albert Einstein. Pp. viii+282. (New York: Philosophical Library, Inc., 1950.) 4.75 dollars.

THIS book is a collection of essays, written by Einstein during the period 1934–50, a continuation of the similar work, "The World as I See It", which covers the years 1922–34. Only a small part of the sixty articles deals with science and its philosophy; others give the author's opinion on the relation of science to religion and to life in general, on moral problems, on politics and the social problem, on the Jewish people and their fate.

It is impossible to give in a few lines an impression of the fascinating and inspiring ideas, expressed in the simplest possible language, which are contained in these essays. A few points may, however, be mentioned. The fourth article, of the year 1937, has the title "Moral Decay", and this term appears later again and again, as a kind of *Leitmotiv*, in all articles dealing with the present political situation, the atomic bomb, the treatment of the Jews, etc. The eighth article, "Science and Religion", contains again a confession which returns in many others: that intelligent thinking plays no part in forming a goal for human endeavour and for ethical convictions—"To make clear these fundamental ends and valuations . . . seems to me precisely the most important function which religion has to perform in the social life of man". What Einstein means by that becomes clear enough from the following considerations: the principles of the Jewish–Christian tradition, which he always, also in other articles, regards as a unit. But just as strong as his acceptance of these "principles"—obviously the ethical teachings of the prophets and of Christ—is his rejection of the belief in a personal God. This opinion is offered in the most simple and humble way as the result of a life's experience and thinking; it is not without connexion with Einstein's deepest scientific convictions.

There are two main articles dealing with these, No. 13 from 1936, and No. 14 from 1940. They reveal much about Einstein's ultimate principles and methods. Every physicist will read them with the greatest interest, as they show how it has come about that Einstein has separated himself from the general trend of modern physics. The summary of No. 13 gives his epistemology in the shortest terms: "Physics constitutes a logical system of thought which is in a state of evolution, and whose basis cannot be obtained through distillation by any inductive method from the experiences lived through, but which can only be attained by free invention". It is just this trust in the power of *a priori* thinking which has led Einstein away from the actual physical research into lofty speculations which have so far little relation to reality. No. 14 contains a clear explanation of the logical situation in quantum theory and of Einstein's objections to the generally accepted statistical interpretation. But this attack is of a milder kind than has ever before come from his pen; it leaves "open to every man to choose the direction of his striving", and it ends with "Lessing's fine saying, that the search for truth is more precious than its possession".

The articles grouped under the titles "Public Affairs" and "Science and Life" show Einstein as a political, social, ethical humanitarian thinker. No. 18 is a confession of socialism, but in No. 31 there is a warning, directed to the blind believers in the social paradise: "Socialism may lead more easily to wars than capitalism because it represents a still greater concentration of power". This problem of war is his main concern, and he sees no other way out but a world government, which he defends and recommends with strong words in many articles, of which No. 31 is the most important one. To this group belongs also his exchange of open letters with four Russian men of science (No. 27) who attack "Dr. Einstein's mistaken notions" by a display of the well-known communistic arguments; Einstein's reply is a model of simple reasoning: first things first—what advantage do you expect from even the most perfect social system if you perpetuate international anarchy with the inevitable consequence of atomic war and general annihilation? War and the military mind are Einstein's real enemies which he relentlessly attacks, for example, in No. 36. But his disgust about the military expression "human material" and the degradation produced by military discipline does not make him a blind pacifist. He appeals to the civilized world to resist aggression, to answer force by force, if necessary, to save its most precious possessions. His words about the suffering of the Jews, "his people", are as moving as his pride in the foundation of a Jewish State.

Einstein's lovable personality, however, is best revealed in some short biographical essays, beginning with Newton and Kepler and leading to Gandhi and Ossietzky (Nos. 38–46). It is not only the achievements of these men which he describes in a masterly way, but also their human greatness. The short sketches dedicated to the memory of Marie Curie, Max Planck, Paul Langevin and Paul Ehrenfest are touching documents of Einstein's devotion to his friends.

MAX BORN

A STUDY OF SOME ODD PERSONALITIES

Very Peculiar People

Portrait Studies in the Queer, the Abnormal and the Uncanny. By Dr. E. J. Dingwall. Pp. 224+11 plates. (London: Rider and Co., n.d.) 18*s*. net.

DR. E. J. DINGWALL has two qualities which make his writing eminently readable: an intense interest in human beings and a profound and unusual scholarship. These qualities are aptly united in this book and its forerunner, "Some Human Oddities", in both of which he makes critical and exploratory studies of strange people and extraordinary happenings. In "Very Peculiar People", Dr. Dingwall considers Emanuel Swedenborg, Johann Jetzer, a seventeenth-century monk around whom centred amazing events, St. Mary Magdalen de' Pazzi, Hadrian Beverland, a Dutch scholar addicted to classical erotica, and Eusapia Palladino, an Italian medium of uncertain authenticity.

In three of these brief biographies the main problems are those of psychical research, and the author, without assaying dogmatic judgment, places before the reader the evidence to be considered. Unfortunately, as he makes abundantly clear, this is insufficient for more than a verdict of 'not proven'. Did Swedenborg in fact possess clairvoyant powers? We cannot tell, and Dr. Dingwall believes that confirmatory facts will never now come to light. He suggests, however, that Swedenborg, along with other

intelligent and otherwise normal persons whom he cites in the appendix, had built up a vast system of internally consistent hallucinations. He believes that this system and its relation to his life as a whole present, quite apart from possible para-normal implications, a valuable field for psychological research.

Was Johann Jetzer the victim of practical jokes, or a joker himself, or were his experiences partly genuine? Here we are in even deeper waters. Jetzer appeared to receive the stigmata and divine visitations of various sorts, but afterwards the prior of his monastery and three of the other monks were executed for perpetrating a blasphemous fraud on him. Nevertheless, the evidence is so conflicting that one cannot be sure that justice was done, and the whole story makes perplexing and fascinating reading.

Was Eusapia Palladino an amazing medium, or an almost equally amazing charlatan? Here we move into more modern times, and the evidence comes from Myers, the Sidgwicks, Lombroso and Lodge; but even so, we are left with an enigmatic question mark.

The other two studies are of a somewhat different character. That of Hadrian Beverland shows the gradual decline of a brilliant and sexually perverted scholar. He was expelled from the University of Leyden for obscene publications and came to England, where he enjoyed some protection and patronage and lived in some comfort until overtaken by chronic illness and a mental disturbance, the onset of which could no doubt have been predicted from his writings and his mode of life. The author suggests that Beverland's decline illustrates the effects of circumstances—he was harshly, if not unjustly, treated in Holland—on the growth of psychological infirmities. This may be so, but we do not know enough of Beverland's youth, and when in his early twenties—at which period we pick up his story—he seems already to have been a meaty case for the psychiatrist.

In his study of St. Mary Magdalen de' Pazzi, Dr. Dingwall shows us a determined woman, who from early childhood overrode every obstacle erected by parents and religious superiors, in the effort to lead what she considered the complete religious life. Dr. Dingwall maintains that her obstinate and inconvenient insistence on her aims, her self-imposed floggings and humiliations, and her frequent ecstasies indicate a highly neurotic character. However, it is recognized that a certain measure of neurosis is an inseparable part of psychic structure and that symptoms of unbalance (as we would understand it) cannot be considered as conclusive when taken out of the context of era and society. We do not know very much about this Saint; but there are certain other factors which always have to be considered in discussing the lives of the religious. The aim of the Christian ascetic is a closer union with God, and the elimination of mental and physical appetites which interpose themselves between the striving soul and its divine goal is a means, but only a means, towards that end. From time immemorial physical austerities, often including beating, have played a part in subordinating the inclinations and the will of the individual to the will of God. Given this long tradition, and given the purpose behind these practices, are we justified in asserting that, save in so far as the finest motives may be slightly mixed, mental and physical austerities are masochistic? Whether or not the potential sexual element vitiates the efficacy of flagellation as a means of self-discipline is another. The saintly Curé d'Ars, himself once an ardent flagellant, later referred to these activities as his "youthful follies" and said that, so far as austerities were concerned, the devil was far more frightened by hunger and lack of sleep than by hair shirts or the discipline.

In conclusion, the story of St. Mary Magdalen de' Pazzi reinforces the belief that the word neurotic, when used indiscriminatingly to qualify odd or inexplicable behaviour, becomes misleading and meaningless.

Dr. Dingwall's aim is to "draw the attention of the student to problems so obscure that any real understanding of them can hardly be said to exist"; and he has done so in these stories, and particularly in the erudite appendixes, in so appetizing and stimulating a way that it would be churlish to accord him only amusement value. His theme is an old one, namely, that human nature is mysterious and that to jump to conclusions about people is to jump to falsehoods; but it cannot be too often reiterated. Dr. Dingwall will probably be blamed for not handing out knowledge and synthetic understanding on a plate; but by stressing the need for knowledge in fields where tradition or superstition usually give ready-made answers, he has performed a greater service.

ADAM CURLE

TREATISE OF ZOOLOGY

Traité de Zoologie; anatomie, systématique, biologie

Publié sous la direction de Prof. Pierre-P. Grassé. Tome 15: Oiseaux. Pp. viii+1,164. (Paris: Masson et Cie., 1950.) 6,000 francs.

ALTHOUGH the present volume is No. 15 out of a contemplated series of seventeen, it is actually only the fourth to be published. There has now been time not only to read the three previously published volumes more carefully but also to use them, and further acquaintance with them has only served to enhance the high opinion formed upon first reading. Vol. 15, which treats of the birds, is fully up to the standard of its predecessors and is larger and consequently more expensive than any of them. Apart from the introductory portion, the text runs to 1,164 pages. It has three coloured plates, of which the first, illustrating types of eggs, is the best; 519 line diagrams, one in two colours; 121 half-tones of photographs; 85 half-tones of drawings; six half-tones of photographs with accompanying explanatory line blocks; and twelve maps. Since a number of these illustrations contain more than one figure, it will be seen that the volume is very well illustrated.

The contributors are J. Bénoit, of Strasbourg; F. Bourlière, J. Berlioz, E. Oemichen, J. Piveteau and A. Rochon-Duvigneaud, of Paris; E. Letard, of Alport; M. Mayaud, of Samur; J. Pasteels, of Brussels; A. Portman, of Basle; E. Matthey, of Lausanne; and the general editor of the series, Pierre-P. Grassé, of the Sorbonne. This list of collaborators not only ensures the authority of the text but also indicates the wide scope of the subject-matter. It is this last point that is perhaps the most striking aspect of the volume as compared with the previous ones, and it is pleasing to see that it reflects the appeal of birds not only to the morphological zoologist and systematist but also to the ecologist, the student of behaviour and the naturalist.

In such a wide survey it is only to be expected that the specialist in a particular field would wish to see his own field dealt with in greater detail. Thus the

cytologist might wish for more than five pages; the student of behaviour might wish for a fuller treatment of the nest building, migration, sense of direction (although this does include a reference to recent work on the possible part played by Corioli's force in this phenomenon), social organization, establishment of territory, etc.; and the morphologist for a fuller indication of the great variability exhibited by the main anterior arteries and so on. When one considers how any of these sections could be expanded without enlarging the volume, one is faced with the problem of what could be omitted. Such a consideration brings a realization of the excellent balance between the breadth of outlook and the great amount of detail contained within the book, and in spite of this it is so clear in exposition that it is easy to read.

There are a few slips such as "co do-mésoblaste" instead of cordo-mésoblaste in the legend of Fig. 353 on p. 488. Other mistakes were noted in reading; but it was later found that these and others were corrected in a list of errata on p. 1154. Each chapter is provided with a select and useful bibliography, and the volume has at the end a detailed table of contents and a full index so that reference to any subject is easy. Unfortunately, the publication of the series is falling somewhat behind the announced dates, which is perhaps only to be expected in such a large undertaking at the present time. The editor and publisher alike are to be congratulated on the production of another most worthy addition to the text-books of general zoology. CHAS. H. O'DONOGHUE

MECHANIZATION OF MATHEMATICS

Calculating Instruments and Machines

By Prof. Douglas R. Hartree. Pp. ix+138. (Urbana, Ill.: University of Illinois Press; London: Cambridge University Press, 1949.) 21*s*.

THE almost exponential rise in the output of scientific and technical research which has been such a marked feature of the past few decades has presented many mathematical problems the solution of which is not possible by known formal methods. The result has been on one hand the development of approximate methods of solution, of which Southwell's 'relaxation' method is an outstanding example, and on the other hand the development of instruments and machines which can carry out speedily and mechanically the complicated mathematical operations involved. The present book, by one who has played a leading part in the utilization of instruments and machines, and in encouraging their development, provides a very welcome addition to the literature of the subject. It is well produced, well illustrated by photographs and diagrams, and contains at the end a list of 122 references.

The book is based on a course of lectures given by the author, so that he has concentrated on those aspects of the subject which interest him most, and on those instruments and machines with which he is most familiar. This has led to the exclusion of reference to devices limited to the solution of a particular problem or equation, and attention is devoted entirely to 'physiology'—what a machine can do and how it can be made to do it—and not to 'anatomy'; although occasional reference is made to components ('hardware') out of which machines are constructed. It is to the user, rather than the designer, that this book will mainly appeal.

The distinction which the author makes between instruments and machines is that which the Americans denote by the adjectives 'analog' and 'digital'. In the former, numbers are represented by some physical quantity; elementary examples are graph paper and the slide rule; but the section of the book devoted to instruments deals almost exclusively with the differential analyser. Machines deal directly with numbers: the human hand and the abacus are primitive forms, but the author is here concerned with the various automatic high-speed digital machines in use or under construction in Great Britain or in the United States. For each class fundamental principles have long been known. Kelvin realized how coupled mechanical integrators could solve differential equations, while Babbage's 'analytical engine' was designed to perform the same kinds of operation as the modern machines. It is to advances in mechanical and electrical engineering, and to increased demand, that modern progress is mainly to be attributed.

Mechanization is no substitute for intelligence. As Lady Lovelace remarked in her account of Babbage's plans, these machines can do only what we know how to order them to do. Moreover, they each have their own idiosyncrasies, their special aptitudes, and their limitations. To take full advantage of the special aptitudes, and to avoid the limitations, may call for considerable ingenuity. The new machines demand a new mathematics—or at least a new approach to mathematical problems—in a word, the cultivation of a 'machine's-eye-view'. This means an analysis of the calculation into a sequence of steps within the capabilities of the machine, or, in some cases, even an alternative formulation of the problem, which allows such an analysis more readily to be made. Instances of this are scattered throughout the text, in connexion with instruments and machines as each is considered, and in the highly suggestive final chapter. The machine's-eye view also means the ability to foresee all the snags.

Although the chapters on the differential analyser show that there have been recent advances both in its design and use, interest to-day is centred rather in the development and use of high-speed automatic digital machines. In the earlier machines of this type emphasis was laid on the reduction of computational labour, and elaborate arithmetical units were built in, such as multiplier-dividers and square-rooters. Experience in use showed a need for much more high-speed storage of both numbers and instructions ('memory'). To-day the tendency seems rather in the direction of quite simple arithmetical units, fundamentally addition units, frequently working in the binary scale, combined with greatly increased storage and control facilities. Multiplication is repeated addition, and the author shows how division can be done by an iterative process involving only addition and multiplication, and that much more complicated calculations can be similarly performed. This lays the emphasis on programming and coding, which take times enormously long compared with those required for the most complicated sequences of arithmetical operations. But programming and coding still seem to need the human brain, training, knowledge, and experience. To keep one of the new machines running at capacity would seem to demand a skilled staff by no means small.

W. G. BICKLEY

Principles and Practice in Organic Chemistry

By Prof. J. Lucas and David Pressman. Pp. xi+557. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1949.) 48*s*. net.

THERE is always some danger that the student who studies the fundamental principles of chemistry under the heading of physical chemistry may overlook the application of these principles to the operations of the organic chemical laboratory. This volume, which is intended as a class-book for a one-year course of practical organic chemistry, has been designed to guard against such a tendency by directing attention to the physico-chemical aspects of the reactions used in preparative work. In addition, therefore, to the usual preliminary description of general laboratory methods, a first chapter is devoted to such topics as the factors influencing reaction velocity, chemical equilibrium, heats of reaction and of formation, changes in free energy and their calculation from available data. Tables of bond energies, resonance energies and free energies are provided.

In subsequent chapters, which include detailed instructions for preparing more than eighty representative organic compounds, thermochemical and kinetic considerations are discussed at appropriate points. Quantities are stated in moles in order to emphasize molecular proportions and also to facilitate the use of alternative substances in the preparations. Notes and questions, calculated to stimulate thought, are added after each experiment.

The selection of experiments is good. The preparations include a wide range of aliphatic and aromatic substances, a few amino-acids, sugars and heterocyclic compounds, and one chromatographic separation. Typical qualitative reactions of each class of substance are given, and a final chapter provides a brief general scheme of qualitative analysis. This is an excellent book which should help to make the organic laboratory a place of sound general chemical education.

G. M. B.

A Review of Recent Advances in X-Ray Analysis, by Sir W. Lawrence Bragg; and The Impact of Radioactivity on Inorganic Chemistry, by Prof. H. J. Emeléus

(Post-Graduate Lectures given to the Oil and Colour Chemists' Association.) Pp. 99+10 plates. (Cambridge: W. Heffer and Sons, Ltd., 1950.) 12*s*. 6*d*. net.

THIS short volume contains the substance of two groups of three lectures, each on the subjects indicated in the title and delivered before the Oil and Colour Chemists' Association in 1947 and 1948. The choice of subjects for these lectures is particularly appropriate since both X-ray analysis and radioactivity have a widespread and growing interest in many branches of science. Moreover, the authors are not only acknowledged experts in their respective fields, but also have the happy knack of presenting scientific information in a lucid and interesting way.

In the first three lectures, Sir Lawrence Bragg describes X-ray analysis by analogy with the optical diffraction grating, with illustrations derived from the structures of many inorganic compounds; he then discusses the variations from the perfect lattice, the structure of some elements and metal alloys, and concludes with a description of work on X-ray analysis of organic compounds. The difficulty and labour of interpreting the results in X-ray work is very well brought out in these lectures. Prof. H. J. Emeléus develops his theme of radioactivity against a historical background and follows this account, which includes atomic fission and the chemistry of the transuranic elements, with a discussion of the measurements, handling and production of radioactive substances. The final lecture describes some of the manifold applications of radioactive tracers in chemistry and biology.

The scope of the subject-matter is necessarily limited in a book of this size, but there is much stimulating information which should appeal to a very wide circle of readers wishing to know more about developments in these two very important subjects. It is pleasant to see a book which is not only very moderate in price according to present-day standards, but is also well bound, printed and illustrated.

F. H. BURSTALL

Trilinear Chart of Nuclear Species

By William H. Sullivan. Pp. 4+chart (in 6 strips) 10½ in. × 194 in. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1949.) 20*s*. net.

THE trilinear chart has been developed from the more familiar charts of nuclear properties in which $(A-Z)$ or A is plotted against Z (where A and Z are the atomic mass and atomic number respectively). It allows the simultaneous plot of A against Z, $(A-Z)$ against Z and $(A-Z)$ against A, so that isotopes, isobars, isotones (nuclides with the same number of neutrons) and isodiapheres (nuclides with the same excess of neutrons) lie on straight lines. All known nuclides are represented by hexagonal panels, those which occur naturally being distinguished from those produced artificially by different background colours. Nuclear physical constants (natural isotope abundance, angular momentum, slow neutron cross-section, half-life, etc.) are shown in the panel of each nuclide. However, such an extensive list of symbols (fourteen in all) requires some reference to the key, and it would perhaps have been advisable to tabulate some of the data (for example, fission yields). The printing on the green field (naturally occurring nuclides) is clear, but a bolder type could have been used to advantage on the lavender background (artificially produced nuclides).

G. B. COOK

Modern Synthetic Rubbers

By Dr. Harry Barron. Third edition, revised and enlarged. Pp. xix+636. (London: Chapman and Hall, Ltd., 1949.) 45*s*. net.

THE third edition of "Modern Synthetic Rubbers" is excellently produced and well illustrated, and contains much new information which has been obtained from B.I.O.S. and C.I.O.S. reports. There is also a new section on compounding.

The author sets out to cover in the very short space of six hundred pages the field of synthetic rubber production and technology from preparation of the monomers to processing of the elastomers. The result is that emphasis is almost entirely on the practical aspects of synthetic rubber production, and at times this leads to a catalogue-like style of presentation. However, the merits of the book more than compensate for the mental indigestion occasionally induced. Mistakes are few, but the reviewer was surprised to find the statement that rubber has a "trans" structure and gutta-percha a "cis" configuration appearing in the third edition of this work.

The book should prove of interest to all who are interested in the study of high polymers.

CHEMISTRY AND BACTERIA*

By Sir CYRIL HINSHELWOOD, F.R.S.

In a culture tube where bacteria grow, some very amazing chemical syntheses go on, with very simple starting materials, and at a great rate. The medium need contain no more than, say, glycerol, ammonium sulphate, phosphate and a few inorganic ions such as potassium and magnesium; and in a few hours a number of cells, which at the start may equal in number the population of the city of Westminster, will have increased so as to exceed the entire population of the continent of Asia. Nor do the cells which perform these feats possess anything in the way of specialized organs. Their internal constitution is not absolutely homogeneous, as special staining methods and observation by the phase-contrast microscope show; but the internal differences are extremely fine-grained, and are, as it were, of a colloid chemical, rather than of a mechanical or physiological degree of differentiation.

Unlike many other organisms, moreover, bacteria, according to the precise conditions of their growth, may vary enormously in size and shape, from nearly spherical cells to long filaments the length of which exceeds their breadth by hundreds of times. The addition to the medium of specific drugs which act separately on growth and division, or even the control of the osmotic conditions, causes these changes, and impresses on us that the cell is, in a sense, an elaborate piece of chemistry rather than a set of organs. The question, then, is how this chemistry is organized. My general thesis is that many characteristic properties of the cell—and these include phenomena which are very typical of the living process in general—are determined by the particular *pattern* of the chemical reactions which take place there. We must examine the nature of this pattern.

In the first place, there is an intimate *linking* of various reactions. The living cell is continually creating order, without, of course, violating the Second Law of Thermodynamics. This being so, reactions involving a decrease of free energy are intimately linked with others which involve an increase. The coupling is possible in virtue of the general tendency of chemical reactions to occur in a series of stages. When we say that the reaction $XY = X + Y$ is coupled with the reaction $B + A_2 = BA_2$, it is not a question of some resonance like that of radio circuits, but of an interlocking of reaction steps such as the following:

$$A_2 = A + A$$
$$A + B = AB$$
$$AB + A_2 = BA_2 + A,$$

with the additional stages:

$$A + XY = AX + Y$$
$$AX + B = AB + X.$$

If this linking of reactions is necessary for the whole energetics of the cell—in order that, for example, the oxidation of glucose should pay for the synthesis of an unstable protein—it is not surprising that it is responsible for a good many special effects as well.

It seems likely that there is in the cell a sort of elaborate production line, in which the products of one enzyme reaction are passed on and constitute the raw material for the next. The unit processes in this series are probably fairly simple. This is a fair inference from the fact that the cell can utilize an enormous variety of substances with its normal apparatus, a versatility which is most easily understood if the unit operations are relatively simple ones, and if the great variety and complexity of the reactions depend upon the pattern of unit processes which is employed to any given end.

Certainly the idea of the long series of consecutive reactions is consistent with the law of growth. Ordinarily, there is, when a number of cells is transferred to a new medium, a considerable *lag* phase, the lag representing the time necessary for the establishment of the stationary state in the sequence of reactions, that is, for the building up of the requisite concentrations of the various intermediates. When this steady state is reached, we have the condition of logarithmic growth. In this phase the amount of bacterial substance increases with time according to the well-known exponential law:

$$\frac{1}{x}\left(\frac{dx}{dt}\right) = \text{constant}.$$

Every single constituent of the cell now reproduces itself identically by the end of a period called the mean generation time.

The implications of this law are important. A living cell consists of an elaborate macromolecular pattern of proteins, polysaccharides and other substances, including those which seem to play a special part in determining its architecture, namely, the nucleic acids. Parts of the protein structure constitute the enzymes, which can often be separated from the cell and made to catalyse a wide variety of reactions in test-tubes. These enzyme reactions occur without the growth of the cell; but the growth of the cell does not occur without the enzyme reactions. In the process of logarithmic growth, every part of the cell substance is reproduced, and it seems almost certain (it is at any rate a hypothesis that we shall make) that the catalytic functioning of the enzyme substance and its reproduction are intimately linked.

This may happen in the following way: suppose we have two enzymes, each using as substrate for self-synthesis something supplied by the other. We can write down the differential equations for the reaction velocities, and we can show that each of the two enzymes will multiply in amount according to the autocatalytic law of logarithmic growth. Thus, although we have, in fact, a system which depends for its functioning upon the linking of two processes, the reproduction of each enzyme is effectively autosynthetic, and we may write as an overall result for each enzyme:

enzyme material + diffusible intermediate metabolite = increased enzyme material + products utilizable in further stages of synthesis

* Substance of a Friday Evening Discourse at the Royal Institution, delivered on November 3.

'Diffusible intermediate metabolite' is written because in general the substrate on which the enzyme acts is not the gross raw material of the medium, but something supplied by another enzyme.

It must be admitted that a system of two enzymes linked in the way I have described is a very drastic over-simplification; yet it does not seem too much to generalize and to suppose that the equation written above applies in general to the growth and expansion of each of the enzymes associated with the various stages in the long and complex synthetic production line. One author has commented adversely on this scheme as an explanation of autosynthesis. He would have been quite right if it had ever been intended as such; but it is not. It is a statement about the dynamical pattern of cell reactions: and its interest lies not in itself, but in its consequences.

We have a series of consecutive reactions, by which a series of enzymatic structures is reproduced. The whole system, in the steady state of growth, comes into a balanced state where formation and consumption of all intermediates are equal. The differential equations for the rates of autosynthesis of the different parts of the cell can now be written down. From them there follows the result that, with a constant set of reaction-velocity constants, the proportions of the different kinds of enzyme material in the cell will settle down to steady ratios. If, however, the conditions are changed so that the relative velocity constants are altered, then the proportions gradually shift during growth until a new steady ratio is attained.

One point may be referred to here in connexion with the linking of use and synthesis in enzyme material. We find that when cells are exposed to ultra-violet light and nearly all are killed, the few survivors contain mutants which are very deficient in growth power in the normal simple synthetic media. When, however, they are left to grow in a struggling fashion for some time, complete recovery eventually takes place.

The change in proportions of the various kinds of enzyme material would appear to be very significant in connexion with the striking phenomena of adaptation. Some cells when transferred, for example, from a familiar source of carbon like glucose to an unfamiliar one like D-arabinose, exhibit at first extreme reluctance to use it; after a period of delay and slow growth, they finally learn, as it were, to utilize it with a maximum efficiency. Again, many cells are inhibited by drugs such as sulphonamides, but after growing rather lamely for a time in presence of amounts of the inhibitor just insufficient to stop growth altogether, acquire what is called 'drug resistance' and then multiply quite happily. By successive increases in the 'training concentration' they may be rendered resistant to amounts a hundredfold that which would originally have stopped all growth.

There is a relatively simple chemical explanation of drug adaptation. Suppose the drug inhibits the action of a particular set of enzymes. At first the synthesis of certain parts of the cell is retarded. Now the cell may very well refuse to divide until certain essential parts have attained some critical magnitude. Multiplication will therefore be held up. By the time it does occur, other parts of the cell, which were not inhibited, have attained more than the usual proportion. These enzymes yield an excess of the intermediates used by those enzymes which the drug blocks. This enhanced concentration can now antagonize the inhibition by the antibacterial drug.

The differential equations of a simple model of such a system show that the enzyme proportions will change until the growth-rate of the cell as a whole returns to its usual value in the absence of the inhibitor. Here, then, we have a mechanism which gives an automatic development of a drug resistance.

Whether, in fact, it operates in real cells, or whether drug resistance is due to chance mutation and selection, is a large question about which I will deal later. For the moment, however, I want to explain chemical hypotheses rather than to defend them, and I will only remark that if such a mechanism plays no part in Nature, we need an explanation why not.

Next we should consider adaptation to new sources of material, such as when the bacteria learn to use nitrogen from ammonium salts instead of an amino-acid mixture, or acetic acid instead of a sugar. As I remarked before, the total sequence of reactions in the synthetic chain is probably divided into relatively simple unit processes, and it is the order and combination of these, and the relative amount of functioning required from each of the relevant enzymes, which varies as we pass from one source of raw material to another.

Various routes are possible from a given raw material to the finished cell material. Examination of the differential equations of a simple model with competing reaction sequences suggests very strongly that, during continued growth, there will, in fact, be established that one which gives the optimum overall rate. The proportion of the various enzymes will settle down to the value which ensures this. Here again it is the dynamical pattern—linking of functioning and synthesis, and the existence of the production line of consecutive reactions—which leads to the automatic adjustment of growth-rate to that which leads to optimum efficiency.

Now adaptive processes are very characteristic of living material. We see that two major types of adaptation are referable to the nature of this dynamic pattern of reactions. They depend upon propositions about consecutive reactions.

Before we turn to the question of alternative biological hypotheses, we will examine one or two other types of reaction pattern, and see what may be derived from them.

One way in which a series of chemical reactions can be linked is cyclically. A given enzyme of a sequence may produce a diffusible intermediate which is one of the necessary reactants or catalysts for one of the earlier enzymes of the same sequence. In this way a cyclical system is set up. The differential equations of a simple system with this mode of linking show that if the initial concentrations of certain intermediates happened to be zero, the cycle could not begin. It would have to wait until primed with a minute trace of one of the substances which could set the cycle in operation. Now when cells which have finished growing are left in the resting state, intermediates are lost by diffusion (and no doubt as the result of other reactions); and if we have any cyclical linking, the result of these losses may be so serious that the steady state corresponding to logarithmic growth will take a very long time to re-establish itself. This may well be the explanation of the very long lag phase which sometimes develops in resting cells. It is, indeed, fair to say that the lag phase develops more rapidly than might have been

expected from the observed rate of decay of individual single enzyme activities.

In a complex pattern of chemical reactions a considerable part might be expected to be played by parallel or competing processes. The existence of these can account for certain remarkable properties of the cell. Just as the pattern of consecutive reactions had a lot to say about the adaptive capacities of the cell, so the pattern of competing reactions explains various phenomena in which, to the casual observation of the natural historian, the cell might be supposed to exhibit choice or purpose. I shall deal with one example, namely, the Pasteur effect, and even this I shall consider mainly in the light of experiments which Dr. A. C. Baskett has recently made in Oxford on the oxidation-reduction properties of *Bact. lactis aerogenes*.

Suppose the cells grow with glucose as the carbon source and with a plentiful supply of molecular oxygen. The glucose is broken down, at least partly by way of pyruvic acid, and much is finally oxidized completely. There is a long series of oxidative steps, and various intermediates are tapped off, aminated and converted into cell material. In the oxidative steps various carriers receive the hydrogen, and they are re-oxidized by a system which ultimately uses molecular oxygen. In the steady state, the re-oxidation of the hydrogen carriers must, of course, balance their reduction.

Now suppose the system is made anaerobic. The aerobic mechanism is replaced by fermentation of glucose; the growth-rate is slower; and the rate of consumption of glucose is much higher than before. In other words, much is wasted. Now suppose that oxygen is passed into the anaerobic culture. The fermentation reactions are now largely inhibited, and although the growth-rate increases, the consumption of glucose actually drops. It seems as though, when the economical aerobic process is available, the wasteful fermentation process is largely inhibited—as though the cell could select which of two kinds of reaction would serve its purpose more efficiently.

What seems to happen is this. The glucose is oxidized in a series of stages, hydrogen carriers being reduced. These must be re-oxidized. If the enzyme system ultimately dependent upon molecular oxygen can operate, it keeps the carrier system X, XH_2 in a highly oxidized form. If there is no oxygen, XH_2 (and in turn other reduced substances YH_2 and so on) build up: there is an autosynthetic expansion of an enzyme system which enables these to be re-oxidized at the expense, not of molecular oxygen, but of wastefully reduced carbon compounds (derived ultimately from the glucose or other primary substrate). Hence the wasteful consumption in the anaerobic process. As soon as air is supplied, the system X, XH_2 (and others in equilibrium with it) now enters in competitive reactions: (*a*) with oxygen and (*b*) with the compounds which would re-oxidize XH_2 in absence of oxygen. The presence of oxygen must lower the concentration of XH_2 and therefore, besides providing the aerobic mechanism, actively cuts out a part of the alternative mechanism which is essential to the fermentation: hence the inhibition of the latter, and the saving of substrate.

Similar explanations based upon the pattern of competing and parallel reactions explain facts such as the inhibition of nitrate reduction by cells which are provided with ammonium salts as a source of nitrogen, and the inhibition of nitrate reduction of certain cultures when they are first made aerobic.

In all the above examples, I have wished simply to point out that certain very characteristic biological phenomena could depend upon patterns of chemical reactions, and have what we may call a kinetic explanation.

That things do so happen is not infrequently called in question by biologists. Here some extremely interesting questions arise which are worth a brief discussion on their own merit.

Let us consider the problem of adaptive changes. We may start with the specific example of drug resistance. According to some biologists, this can concern the chemist only in a very indirect way. Drug-resistant cells are supposed to arise not by any interaction of cells and their environment, but by chance mutations, the only effect of the medium being to select those individuals which, in fact, happen to be able to grow in it, and so to eliminate the normal non-resistant cells. The chief reason for this view was originally the belief in the non-inheritance of acquired characters. Once trained to resist, say, a sulphonamide drug, bacteria pass on the capacity to their progeny for many generations. Now it is, of course, well known that characteristics which animals and plants have acquired by nurture, exercise or mutilation are not passed on to their descendants (at least in the vast majority of cases). To suppose that an acquired drug resistance could be heritable was deemed Lamarckian and therefore heretical. Hence, it was argued, the apparently adaptive modifications must be the result of chance modifications in genic material not directly affected by the environment.

This is not really a logical view at all. Unicellular organisms are different from complex animals or plants in some important ways. They are directly exposed to the action of the environment, and all their chemical reactions are subject to the direct intervention of any substances that the medium may contain. Complex organisms, on the other hand, have their reproductive processes localized in special cells, so that chemical influences of the medium probably never reach them. Modifications of animals and plants are far more likely to be due to Mendelian recombinations of relatively unchanging genes than to chemical changes in the cell material as a whole. This does not mean to say that such changes may not play very important parts in the behaviour of bacteria.

There is really no antagonism here: it is simply a question of the relative importance of two kinds of process (both of which might be expected to occur in one or another kind of example). The chemist, therefore, is not, I think, being heretical if he makes hypotheses about adaptive changes in unicellular organisms. It is a matter for detailed experiment to find out what does, in fact, happen. Here a great many subtle considerations arise.

It will be useful to examine some arguments on the two sides: whether, namely, adaptive changes in bacteria are provoked chemically in some such way as I have discussed earlier, or whether they depend upon chance mutations.

One of the arguments now much in vogue is a statistical one originated by Luria and Delbrück, and which is briefly as follows. A large number of different cultures of an organism are plated out in presence of an antibiotic such as penicillin. The number of resistant colonies which grow shows very wide variations. On the other hand, a corresponding number taken from a given culture does not show this wide spread. It is argued that this must be explained

by random mutations. If a mutation occurred late in a given culture, there would be few mutants in a sample tested by plating: if it had occurred early, there would be many. This argument is open to a grave objection. A recent paper by Eriksen has shown that the spread of the results is a peculiar function of the exact conditions of the test. Ten samples of different cultures cannot be statistically comparable with ten from the same culture, because the ages of the ten different cultures cannot possibly (owing to fluctuations in growing times) be as closely controlled as those of ten parallel samples taken from a given culture. In any event, a variability in the initial power to survive penicillin treatment would not preclude an adaptive development of this resistance.

Time does not permit a detailed discussion of this interesting topic, but I have perhaps said enough to indicate what an interesting statistical problem is presented.

Another line of argument is the following. When certain bacteria are inoculated into a new medium (say D-arabinose instead of glucose), there may be a lag of several days before they grow. According to one view, this lag represents the time required for the setting up of the new steady state in the reaction sequence; according to the other, it represents the time required for the multiplication of the minute proportion, say $1/10^6$, of the mutants already there. Now suppose we transfer 10^4 cells. If there were only the small proportion of mutants supposed, then there would be only a 1/100 chance that the inoculum contained a mutant. According to the adaptation theory all might grow. Tests with transfers of different sizes show that, in fact, in some examples practically all the transferred cells do, in fact, grow given the requisite time. This is a *prima facie* case for the theory of direct adaptation.

There is an objection, however, which is difficult to counter directly. The new medium may contain enough impurity to support a minute growth of non-mutant cells. Although this growth may be below the threshold of visibility, it may be enough to allow the throwing off of some extra mutants to account for the final positive result. This explanation will only work if some rather *ad hoc* assumptions are made about the impurities which various media do or do not contain; and without much direct evidence we may say, therefore, that the objection is in a sense an unfair one, though none the less difficult to refute. We must turn, therefore, to other lines of evidence.

Interesting considerations arise in connexion with the stability of adapted forms. I will quote a specific example. *Bact. lactis aerogenes* shows an initial lag of 30–40 hr. when it is first cultured in the sugar D-arabinose. If this lag were, in fact, the time required for the multiplication of the mutants already there, it would be necessary to assume about one mutant in about ten million cells. This fraction does not increase with the history of the strain; so we must suppose that the very small proportion is due to an equilibrium between mutations and reverse mutations. Since the equilibrium proportion is very small, the reverse mutation-rate has to be assumed very large. On this basis one can calculate how fast the cells should lose the acquired power of utilizing D-arabinose: the answer is that they should revert at a considerable rate. In fact, the adaptation is very stable.

To retain the mutation hypothesis, special assumptions have to be introduced, and these are not of a very probable kind. Moreover, the ease of reversion of adapted cells is, in general, a very marked and regular function of the thoroughness with which they have been originally trained. To save the mutation theory here we have to postulate successive mutations, the first to give a new property and the second to render that property stable. On the other hand, various simple chemical hypotheses about stability of adaptation can be made, and, while it would not be fair to say that they encounter no difficulties, I think the difficulties may be said to be much less serious.

Another line of evidence which I find interesting is the fact that the various kinds of adaptation are additive. By suitable procedures one can train *Bact. lactis aerogenes* to utilize lactose with optimal efficiency: the resulting strain is called *L*. Similar strains can be trained to use sucrose (strain *S*) or maltose (strain *M*) with maximum efficiency. By suitable procedures one can prepare strains which utilize pairs, or all three with optimal efficiency: *LS*, *LM*, *MS* and *LMS*. If all the capacities are acquired by chance mutations, as they might *possibly* be, one can only say that the role assigned to chance is a rather remarkable one.

There is also a quantitative side to the matter. If we train bacteria to resist a drug such as proflavine (diamino acridine), the resistance will be exactly graded to the concentration at which the training was carried out. A complete set of strains can be prepared with resistances exactly related to the concentration of the drug in which they were adapted. According to the kinetic picture which I mentioned earlier this is very natural, but according to the mutation theory it implies a whole series of mutations of a numerous and complex character.

To sum up the situation, I would say that in respect of these particular phenomena the chemical theories have the advantage, not perhaps in the absolute, but at least in the light of the famous scholastic principle *Entia non sunt praeter necessitatem multiplicanda*.

No reasonable man of science could possibly deny that genic re-combination phenomena play an enormously important part in biology; but that alongside these, effects depending ultimately upon the dynamical pattern of the cell reactions also play their part, is a hypothesis which I think the chemist may find fruitful for some time yet.

SEPARATION AND PURIFICATION OF ENZYMES ASSOCIATED WITH INSOLUBLE PARTICLES

By R. K. MORTON*

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RECENT studies on the distribution of intracellular enzymes have clearly demonstrated that many are closely associated with well-defined cell components[1,2]. Autolysis and digestion with lipases, trypsin and papain have been used for bringing into solution several enzymes associated with these insoluble particles, while sonic vibration[3,4] and treatment with sodium deoxycholate[5] have enabled some

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separation of components of mitochondria. These methods have limited application, and all are more or less undesirable for enzyme purification. Some enzymes are altered or destroyed by proteolytic procedures, while contaminating products are formed during digestion; sonic vibration may only cause increasing dispersion of cell material, and there is considerable possibility of denaturation; and sodium deoxycholate may promote 'solution' through micelle formation, as is the case for most detergents.

The application of alcohols (propyl and shorter chain-length) to the separation of the water-soluble enzymes of muscle has recently been investigated[6]. The controlled use of *n*-butyl alcohol has now been found to enable ready solution of a wide range of enzymes from insoluble particles into distilled water or dilute buffers, apparently by removal of lipid and other material from cell structures. The method appears capable of considerable extension and development, not only for the separation and purification of enzymes from animal and plant tissues, yeasts and bacteria, but also for the isolation of other biological materials.

Specific Action of *n*-Butyl Alcohol

Investigation of several alkaline phosphatases of animal materials suggested that true solution of the enzymes was prevented by association of the protein with lipid material. Of a wide range of organic solvents tested with varying procedures, *n*-butyl alcohol alone appeared capable of removing the lipid sufficiently to enable quantitative solution of the active enzymes in the aqueous phase.

This alcohol is ideally suited to enzyme purification. Low solubility permits saturation of the aqueous material without loss of activity (in the case of enzymes so far investigated); lipids, organic esters and other materials soluble in the excess alcohol are removed from the aqueous phase; protein denaturation can be controlled by use of proper extraction conditions; and enzymes are frequently recovered almost quantitatively in clear water solution. The alcohol in the aqueous phase may be removed by dialysis, or the enzymes precipitated directly with other organic solvents. Further purification of the enzymes is then possible using normal procedures.

Where an enzyme is unstable to treatment of wet material with the alcohol, it may be useful to remove lipids by extraction of the acetone-powder or vacuum-dried preparation with dry *n*-butyl alcohol at low temperature. Following appropriate removal of the alcohol, the protein may be brought into solution in distilled water or buffer. Freezing the material with *n*-butyl alcohol at very low temperatures, such as those used with other treatment of plasma proteins[7], has not been found of any advantage with enzymes so far studied.

Method for Enzyme Purification

The procedure adopted for the purification of alkaline phosphatases is given in detail since it illustrates the general method. Appropriate modifications have been introduced for the extraction and purification of other enzymes.

Phosphatases

Alkaline Phosphomonoesterase. This enzyme is closely associated with the cell nucleus in animal tissues[1,2] and in milk with insoluble lipid material[4,9]. Prolonged autolysis and tryptic digestion have both been used previously as essential preliminary procedures in purification, although some of the enzyme may be obtained in solution by other methods[10].

Intestinal Phosphatase. Approximately 3 litres of calf intestinal mucosa is dispersed with vigorous mechanical stirring in distilled water to a total volume of 10 litres. Acetate buffer (2*N*, *p*H 4·5) is added and the precipitate at *p*H 5·1 separated in a 13-litre serum centrifuge (2,000 r.p.m., 20 min.). This precipitate, consisting largely of cell fragments, is washed with 8 litres of distilled water and again dispersed in distilled water to give a suspension of 5 litres, *p*H 5·2–5·5.

Slow addition of 2 litres *n*-butyl alcohol (laboratory reagent grade) with vigorous mechanical stirring at room temperature forms an emulsion and precipitates a considerable portion of inactive protein. The emulsion is rapidly stirred and heated to 32° C. for 5 min. by immersion of the stainless-steel container in a water-bath held at 40° C. When centrifuged (2,000 r.p.m., 20 min., in 1-litre glass cups) the material separates into distinct layers comprising a heavy precipitate, a clear yellow aqueous zone overlaid by light particles, and a surface layer of excess *n*-butyl alcohol containing dissolved material. The latter is readily removed by suction and the butyl alcohol recovered by distillation. The aqueous phase is conveniently removed by suction through a narrow glass tube and filtered if necessary.

The solution contains about 50 per cent of the original phosphatase of the mucosa, and has a Q_p of about 10^4. Further purification by usual methods has yielded final preparations with a Q_p of between 5×10^5 and 10^6, and these are thus about five times more active than any previously reported[11]. Recovery is between 20 and 30 per cent. It is noteworthy that the enzyme may be quantitatively eluted from a variety of adsorbents at any stage after true solution is obtained by the treatment with *n*-butyl alcohol (compare ref. 12).

Other Phosphatases. Serum ('buttermilk') obtained from cream of 60–70 per cent butterfat is treated with excess *n*-butyl alcohol at *p*H 5·2 and heated to 35° C. for 5 min. to give a clear yellow phase containing the alkaline phosphatase of milk in true solution, free of casein.

Further fractionation and adsorption procedures have given a preparation with a Q_p of 10^5. This represents a purification of approximately 5,000 times as compared with normal milk, and the preparation is about a hundred times more active than any previously described[9,13]. Appropriate modifications of the above procedures have been used for the separation of phosphatases from mammary gland, kidney, liver and yeast, substantiating the general applicability of the technique.

Pyrophosphatase. The enzyme of optimum *p*H 7·6 (for hydrolysis), closely associated with alkaline phosphomonoesterase in animal tissues[14], is readily brought into aqueous solution from intestinal mucosa and kidney by the use of *n*-butyl alcohol.

Adenosine Triphosphatase. It appeared possible that enzymes specifically catalysing the breakdown of adenosine triphosphate might be closely associated with lipids, as are animal monophosphoesterases. A magnesium-activated adenosine triphosphatase of muscle has been reported to consist of such an

association of protein and phospholipid in a complex the integrity of which is essential to enzymic activity[15]. An adenosine triphosphatase has also been reported to occur in isolated mitochondria[16].

Treatment of rabbit muscle and liver preparations with *n*-butyl alcohol permits isolation of the adenosine triphosphatase activity in the aqueous phase. Apparent separation of the calcium-activated enzyme from rabbit-muscle myosin into a clear salt-free solution has now been achieved by extension of the same method. It may be that this enzyme is also associated with phospholipid or similar material, which is always co-precipitated when myosin is prepared by the usual methods. It is noteworthy that the myosin remaining after *n*-butyl alcohol treatment appears undenatured as judged by the ready solubility in 0·5 *M* potassium chloride (*p*H 7·2).

Peptidases

γ-Glutamyl Transpeptidase. An interaction between glutathione and free amino-acids with formation of γ-glutamyl peptides ('transpeptidation') has recently been described[17]. The enzyme catalysing this reaction is probably identical with one in pig kidney described as associated with a "lipoprotein of high molecular weight"[18]. By *n*-butyl alcohol treatment it has now been obtained in clear solution from both sheep and ox kidney.

The homogenates at *p*H 6·8 are treated with excess *n*-butyl alcohol, centrifuged, and the clear aqueous layer containing the enzyme dialysed against 0·05 *M* phosphate buffer, *p*H 7·4. These procedures are carried out at low temperature (0°–2° C.).

By subsequent fractionation, water-clear active preparations of low protein content have been obtained. Similar treatment of minced mammary gland from a lactating cow has also given clear preparations found to contain an enzyme with 'transpeptidase' activity.

Cysteinyl-glycine Dipeptidase. A recent paper has reported the separation of this enzyme from its physical association with the transpeptidase enzyme of pig kidney[19]. It is noteworthy that shaking with chloroform is an essential part of the purification method therein described. By treatment of ox kidney with *n*-butyl alcohol, this enzyme may also be readily brought into clear solution and purified by subsequent fractionation procedures.

Dehydrogenases

Succinic Dehydrogenase. This important enzyme is readily obtained in mitochondrial preparations and has been intensively studied in such colloidal suspensions. A brief account of the extraction of a small amount of the enzyme from liver mitochondria of guinea pig has been published[20], but no further details of this work have appeared. Attempts to repeat the extraction procedure with calf-liver and horse-heart preparations were unsuccessful in this laboratory. A recent paper[3] has described further investigations designed to obtain this and other enzymes in a soluble form by sonic vibration of rat-liver mitochondria. A similar method has been used to obtain an enzyme preparation from *Corynebacterium diphtheriæ*[21].

When a mitochondrial preparation from horse-heart[22] is treated with *n*-butyl alcohol, almost quantitative recovery of the succinic dehydrogenase is obtained in the clear aqueous layer, as follows. The precipitate at *p*H 5·6 is washed with cold distilled water or 0·001 *M* sodium bicarbonate solution, adjusted to *p*H 7·5–8·0 with 0·02*M* sodium bicarbonate solution containing 0·85 per cent sodium chloride, and stirred thoroughly with excess *n*-butyl alcohol at low temperature (0 to −2° C.). The emulsion is held at this temperature for ½–1 hr., and occasionally stirred to promote lipid extraction. After centrifuging at low temperature, the clear aqueous layer containing the enzyme is removed and dialysed at 0° C. against 0·01 *M* sodium bicarbonate and afterwards against distilled water. For complete recovery of the enzyme the extraction is repeated.

Preliminary fractionation with ammonium sulphate has yielded very active preparations as determined spectrophotometrically by reduction of potassium ferricyanide. The enzyme, which has also been obtained in solution from other animal tissues and from yeast, is being further purified and its relation to the cytochrome system is being studied.

Other Dehydrogenases. Mitochondrial preparations from ox kidney and liver and rabbit skeletal muscle, and homogenates of rabbit brain, have been treated with *n*-butyl alcohol, and a number of other dehydrogenases, formerly recognized only in association with insoluble particles, have been obtained in solution. Particular attention is being paid to the pyruvic, choline and α-glycerophosphate dehydrogenases.

Xanthine Oxidase. Treatment of the milk protein with ether[23] and digestion with pancreatic lipase[24] have previously been used to free this enzyme from associated lipid material. During the preparation of alkaline phosphatase of milk using *n*-butyl alcohol, a red-coloured fraction was obtained. This showed an absorption spectrum similar to that of xanthine oxidase, and had xanthine dehydrogenase activity, although the intensity of colour, which was entirely reducible with sodium hydrosulphite, suggested association of other material.

Transaminase

A transamination reaction between phenylpyruvic acid and L-glutamate has recently been described and shown to be strictly associated with insoluble particles of rat liver[25]. By treatment of a washed homogenate of calf-liver with *n*-butyl alcohol, a clear solution containing the transaminase system of the insoluble particles has now been obtained.

Esterases

Insect Esterase. Treatment with *n*-butyl alcohol has been employed for the purification of an esterase hydrolysing ethyl butyrate and occurring in meal-worm larvæ[26]. The crushed larvæ contain considerable lipid which is readily soluble in cold *n*-butyl alcohol, thus permitting aqueous solution of the enzyme.

Intestinal Cholinesterase. The enzyme of calf intestinal mucosa[27] may be obtained in solution by *n*-butyl alcohol treatment, and this method is probably applicable to the separation of cholinesterases from brain and other tissues.

Crystalline Proteins

During acetone fractionation of clear solutions obtained after treatment of calf intestinal mucosa, liver and kidney with *n*-butyl alcohol followed by prolonged dialysis against distilled water, several

proteins have been obtained as crystals. The possibility of some of these having enzymic activity has not been eliminated. A protein obtained from calf liver at the 30–33 per cent acetone fraction as slender needles, after recrystallization gave positive tests for hexosamine and may be a glycoprotein.

Discussion

The above results obtained with a wide range of enzymes associated with insoluble materials indicate that *n*-butyl alcohol is an ideal solvent for the separation of biologically active proteins. The specific action is largely due to the removal of lipids, particularly of phospholipids: alkaline phosphatase and xanthine oxidase of milk are both associated with such insoluble material; animal tissue mitochondria contain up to 40 per cent (dry weight) of lipids, most of which are phospholipids[28]; while pea seeds, yeasts and bacteria may contain similar large amounts. All these materials have been successfully treated with *n*-butyl alcohol, and active enzymes obtained in clear solutions.

The solvent action of the two-phase system for organic esters, particularly phosphate esters, is also probably of importance. Considerable organic phosphate is found in the residue obtained by distillation of *n*-butyl alcohol used for extraction of milk, calf-liver and certain mitochondrial preparations. It is conceivable that these esters are closely bound with lipid in maintaining structural integrity and controlling permeability of cellular membranes, so that their removal enables release of certain proteins into solution.

The straight-chain configuration apparently favours aqueous solution of enzymes, perhaps through interaction with the protein side-chains, since the *n*-butyl alcohol is superior for this purpose to both the *iso*- and secondary butyl alcohols. The latter are also more toxic to enzymes. For promoting protein solution the completely water-miscible propyl and lower alcohols are relatively ineffective, as are the amyl and higher alcohols of low water-solubility, although the two-phase system of water and alcohol appears to be essential for satisfactory treatment of most wet preparations.

A specific ability of *n*-butyl alcohol to induce separation of lipoprotein complexes has previously been observed during an investigation of the tanning process in the insect cuticle[29]. Evidence from this work suggested that *n*-butyl alcohol competes for the polar side-chains of the protein, so that the alcohol effectively replaces displaced lipids. Such a competitive action may well be of importance in causing dissociation, not only of lipoprotein complexes but also of protein–protein complexes, and thus in favouring ready separation of enzymes.

The interaction of the alcohol with the enzymes in solution is influenced by temperature, hydrogen ion concentration, ionic strength, and specific ion effects, as already shown for other organic solvents used for enzyme[6] and protein purification[30]. The occurrence of crystalline proteins, as previously noted, confirms that denaturation has been considerably eliminated in the methods described, which contrast with the protein-denaturation procedures employing chloroform together with butyl and higher alcohols for the separation of mucopolysaccharides and mucoids[31].

The relative importance of the several factors mentioned is now being examined in detail to determine the mechanism of action of *n*-butyl alcohol and to permit of a more fundamental approach to the problem of enzyme solubility.

The enzymes so far obtained in solution are being further purified and intensively studied in this Department, while the work is being extended to the separation of other enzymes of mitochondria, particularly those of the cytochrome system and citric acid cycle. Apart from the evident advantages of study of these enzymes in solution and in purified preparations, it is apparent that the removal of large amounts of inactive materials, especially as found in certain plant tissues, may permit of *in vitro* demonstration of reactions at present only recognized in intact tissues or complete organs. Undoubtedly much of the material removed by proper treatment with *n*-butyl alcohol could effectively inhibit certain enzymes in homogenates. The comparatively ready extraction of proteins from yeast and bacteria treated with *n*-butyl alcohol at low temperature suggests the value of the method in obtaining cell-free enzymes from these organisms.

While the method has been presented in some detail as applied to enzyme proteins, the possible use for the purification of other biologically important material is apparent. Lipoproteins still present many problems connected with lipid association. Application of *n*-butyl alcohol to the purification of the lipoprotein complex of lung is being investigated. Hormone purification and steroid metabolism are other fields in which this technique may prove of value.

I wish to thank Dr. M. Dixon for his encouraging interest and advice throughout this work, and Dr. G. Weber for helpful discussion on physico-chemical aspects. I am indebted to a number of fellow-workers, particularly to Mr. F. J. R. Hird, for determining enzyme activities of certain preparations.

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JUBILEE OF THE SCIENCE MASTERS' ASSOCIATION

By G. FOWLES

Chairman of the Association, 1940–45

THE Science Masters' Association celebrates its jubilee at the annual meeting being held in London during January 2–5. A commemorative dinner has been arranged at which many distinguished guests will be present.

At the time of its foundation in 1900, membership of the Association was open to masters in public schools only. However, in the eventful year 1918 the more progressively minded of the Committee came to the conclusion "that we owe it to the good of the nation to accept members from other types of school". In 1919, against much opposition, these men of vision won their point. Rarely has such a democratic gesture proved so great a success. The Association grew rapidly; the membership, less than 350 in 1919, had by 1939 risen to 2,600; in all directions there was recognition and welcome. The universities, old and new, the great scientific institutions and the leaders of industry alike, offered help and hospitality. In turn, a succession of the leading men of science in Great Britain willingly accepted office as president and gave of their best. Probably the single-minded aim of the Association, "to promote the teaching of science in schools", and the absence of trade union status and outlook and of any ulterior motive, had something to do with this; for it allowed public men and officials to attend the meetings and express their views. Prominent among such regular visitors in the early 'twenties were Sir Richard Gregory, Sir Harold Hartley, and Profs. H. E. Armstrong and A. Smithells. With their scholarship, their wider outlook and their warm attitude to the cause, their very presence at the conferences did much to raise the level of discussion and to broaden the outlook. From the beginning, H.M. inspectors of schools were enthusiastic and helpful members. The social contact permitted of informal talks which made official negotiations all the smoother. In recognition of their friendly help, the Association has made four of their number honorary members: E. G. (now Sir Graham) Savage, F. B. Stead, C. J. R. Whitmore and A. R. Marshall.

Since it is generally recognized that science is a dominant factor in the world of to-day, teachers instinctively realize that their attitude to it and to its teaching is of vital significance. Yet science is a comparatively new subject in the schools. The opportunity, therefore, of exchanging ideas and experience and of discussing methods of teaching afforded by the Science Masters' Association furthered its rapid growth. Still, I believe that much of the present prosperity is due to the sound planning of the early leaders and their successors. On four of these the Association bestowed its greatest honour: each in his day filled the office of president. They are Archer Vassall and C. L. Bryant, both of Harrow, C. E. (now Sir Cyril) Ashford of Dartmouth, and G. H. J. Adlam of the City of London School. These pioneers took a large share in building the two pillars of the Association, namely, the annual conference and the Association's journal, the *School Science Review*. The latter owes much to the ability and devotion of its founder and first editor, G. H. J. Adlam. The first issue, a booklet of 32 pages, appeared in 1919; the sixty-fifth, issued in 1939, contained 160 pages of reading matter. Adlam was responsible for the first 103 numbers, and on his death in 1946 Spenser R. Humby, of Winchester, was appointed editor. All are pleased that while he retains the arrangement and keeps to the tradition of Adlam, he from time to time adds ideas of his own. Most of the contributions are by members; but, when necessary, outside specialists are called upon to give expositions of difficult problems and of new advances. In addition, the journal contains the presidential and other addresses, records of meetings, descriptions of school-made apparatus and of new experiments. Those experiments recorded in the journal which seemed worthy of perpetuation have been suitably arranged in book form; so far, five of these "Science Masters' Books" have been published.

Adlam's achievement in raising the *School Science Review* to the foremost place among journals of its class was so remarkable and so beneficial to science teaching that news of it spread afar. In 1934, when his work was recognized by the award of the O.B.E., the Association was indeed proud. Hundreds of members helped Adlam by writing articles, reviewing books, giving specialist advice and answering the questions of correspondents. If from these I single out for remembrance the name of Bernard Neville of William Ellis School, I am sure none would demur. Neville had a brilliant brain, and his physical affliction—he was a cripple—seems to have imbued him with a more than ordinary desire to serve his fellow-men.

The annual conference of the Association, usually held in the Christmas holidays, lasts for three days. The president delivers his address and well-known science specialists give lectures, so that the conference becomes in part a refresher course. Members exhibit school apparatus and show novel experiments and devices. The Committee arranges discussions and visits to places of scientific interest. Publishers and instrument makers display their wares and demonstrate the working of new apparatus. Opportunities for social contacts and for informal discussions abound. The many who attend these conferences speak appreciatively of the stimulation and enjoyment they bring. Among those attending the Jubilee Conference are the president, Sir Robert Robinson, the Dean of St. Paul's, who is to give an address on "Knowledge, Religious and Scientific", A. H. T. Glover, who is to deliver the Science and Citizenship Lecture, and Profs. C. K. Ingold, C. A. Coulson and J. Z. Young.

The Science Masters' Association has already done much good work; still more work and difficult problems lie ahead. These latter include: the problem of science for the classical sixth form, the perennial question of the admission of women members, the role of the broadcast and the film strip in schools, and the machinery for running the Association. The recent Education Act created the secondary modern schools, and teachers in them were immediately welcomed into the Association and offered the help of its accumulated experience. In a few years time, this new class could easily outnumber the present members and determine the policy of the Association—a problem indeed. Yet I look to the future with confidence. In H. P. Ramage, of Gresham's School, Holt, we have a leader of tried worth. Already in one office or another he has served for fourteen years. During the War he did more than any other to keep the Association in being; it is fitting that in its jubilee year he should be chairman of the Association.

NEWS and VIEWS

Unanswered Questions

A NEW experimental venture in the information field has recently been undertaken by the Intelligence Division of the Department of Scientific and Industrial Research. A bulletin entitled *Unanswered Questions*, the first two issues of which have so far appeared, is being made freely available to interested organizations and individuals in Great Britain and overseas. The purpose of the new bulletin is twofold. As the title suggests, it is hoped through the medium of *Unanswered Questions* to obtain answers to specific scientific and technical questions when the normal processes of searching have apparently failed. Readers of the bulletin are invited to supply any information which may help to answer questions listed in the bulletin and also to submit questions of their own. Where the questions can be answered from sources of information known to the Department, the inquirer is put in touch with the source in the normal way.

Fourteen questions were listed in the first number of the bulletin, and some information was quickly received on eight of these. Attempts were then made to discover why the original inquirers had failed to find the answers. It is hoped that by studying a number of questions in this way it may be possible to collect data on the adequacy of existing methods for disseminating information. This is the second and perhaps more important purpose of the new bulletin. The success of this experiment depends, of course, on the supply of questions. It is hoped, therefore, that all who have scientific and technical questions which have not been answered by the normal process of searching will send them to the address given below. The source of any question is not disclosed. Copies of *Unanswered Questions* may be obtained free of charge from the Department of Scientific and Industrial Research, Intelligence I, Charles House, 5–11 Regent Street, London, S.W.1.

Industrial Time-Study Methods

As part of its programme of research into practical problems, the Nuffield Foundation is sponsoring an independent and disinterested scientific investigation into the techniques of stop-watch time-study practised in industry to-day. A steering committee of physiologists, psychologists, statisticians and experts in management has been appointed, under the chairmanship of Sir Frederic Bartlett, to guide the inquiry; and Mr. Winston Rodgers, head of the Department of Industrial Engineering at Acton Technical College, London, W.3, has directed the first stage of the investigation, now completed. This stage was confined to testing the consistency of results obtained from existing methods of stop-watch time-study. An experiment was designed, and carried out at Acton Technical College, whereby a group of time-study practitioners, employing conventional time-study techniques, were set to establish time-values for the same operations. The results of this experiment have proved conclusively that, working under laboratory conditions with variables as far as possible reduced and controlled, the final time-values set by the practitioners showed a high degree of inconsistency. In the light of these results, the steering committee is planning further work on the inquiry, with the long-term objective of evolving scientifically valid principles of work-measurement, as an alternative to existing practices. It is not intended, at this stage, to publish details of the results of the laboratory experiment at Acton Technical College, though it is likely that they will form part of any final report on the inquiry that may be produced. Mr. Winston Rodgers will be willing to give more detailed information on the conduct of the experiment to those who may be interested.

Social Sciences Research Register

IN the social sciences it is difficult for the research worker and those concerned with the direction and administration of research to keep in touch with the development of ideas and trends in this field, or to avoid duplication with work being carried on elsewhere. The boundaries of the social sciences are undetermined, and the data are scattered, much, for example, being the by-products of administration or to be found only in the archives and documents of public, semi-public or private organizations of all kinds. The "Register of Research in the Social Sciences in Progress and in Plan, No. 7: 1949–50, with a Directory of Research Institutions", prepared annually by the National Institute of Economic and Social Research under the editorship of Feodora Stone (pp. 174; London: Cambridge University Press, 1950; 15*s*.), therefore serves a useful purpose by providing a catalogue of research in progress at universities and research institutes, or which is being carried out by research groups or the learned and professional societies. Though it cannot hope to be exhaustive, and though it does not cover the research of private persons working on their own, or of commercial, industrial, political and similar bodies, the catalogue of work in progress is nevertheless very impressive, a total of 1,265 pieces of research being recorded and described. There is also a useful directory of research institutions, giving essential information on nearly sixty organizations. The register includes particulars of the work of research students proceeding to the Ph.D. degree, and this should prove a useful feature for those responsible for the work of postgraduate students. The volume is well indexed, there being separate indexes for institutions, names of research workers and subjects of research.

Productivity and Management

IN his address on "Productivity", at the recent annual general meeting of Lever Brothers and Unilever, Ltd., Sir Geoffrey Heyworth made a valuable and practical contribution by describing the experience of his firm in tackling this problem. Dealing with the human factor in productivity, he emphasized that wherever individual performance is inadequate, it is wise to look at the methods of supervision in some detail. Day-to-day procedure and the level of understanding on the part of supervisors may vary considerably. In the experience of his firm, he stated, a good manager's efforts to increase efficiency, after mutual consultation, are scarcely ever frustrated by resistance from the workers. Neither workers, nor office personnel, like to work long hours, but while they are on the job they are generally willing to work. Sir Geoffrey's firm is in a position to compare conditions in different countries because of its manifold activities, but it has not found any important differences between the workers of various European nationalities as far as willingness to work and potential performance are concerned. Generally speaking, there are two motives for the acceptance by workers of production study: namely, that it saves energy and

that it brings in extra money. It follows that the worker must be guaranteed that his wage will be maintained even if the amount of physical effort required is less than was necessary before the improvements were effected. The acceptance by all concerned of any new work tasks is vital and can only be achieved by explaining in detail to everybody exactly how the measurements have been arrived at; no trouble is too much to take to ensure full understanding.

British Instruments for Radioactive Measurement

THE Scientific Instrument Manufacturers Association, in conjunction with the Atomic Energy Research Establishment of the Ministry of Supply, have prepared an illustrated brochure describing the principal instruments required for investigations using radioactive substances that are manufactured in Great Britain. The rapid developments in this field during the past five years are emphasized by the large number of instruments recorded in the thirty-odd pages of the catalogue. The catalogue is prefaced by a short description of radioactive measurement techniques and followed by advertisements of consultants in this field. The index tabulates nearly forty kinds of instruments, principally electronic, many of which are now manufactured by more than one supplier. The brochure shows that Great Britain can offer a range and variety of such instruments which will compare favourably, both in cost and performance, with any competitor. This catalogue is of particular importance to overseas consumers contemplating purchase in Great Britain of equipment for radioactive measurements. For this reason, it is unfortunate that full technical specifications and performance data for each instrument are not provided. Many instrument manufacturers still fail to achieve a clear and factual, and concise but comprehensive, specification of their products. The rapid development in instrumentation in this field encourages one to hope that further editions will be issued. The brochure is available free of charge from the Scientific Instrument Manufacturers Association, 17 Princes Gate, London, S.W.7.

Exploration of a Coast-line of the Antarctic

ONE of the last remaining gaps in the known Antarctic coast-line was the south-western coast of the Weddell Sea. Various attempts have been made to reach this by sea, but it is now realized that the currents of that Sea pack the ice against the coast and prevent any access, at least without extreme peril to the ship. This stretch from about lat. 68° S. to lat 70° S., the work of the joint sledge journey of the Falkland Islands Dependencies Expedition and the United States Antarctic Research Expedition, is shown in a map, accompanying an article by Mr. D. P. Mason, in the *Geographical Journal* of April–June this year. Large glaciers seem to pour into deep indentations, and the Larsen Ice Shelf continues in a narrow form well to the south. From the base at Stonington Island in Marguerite Bay the route was first across the peninsula of Graham Land and then southward over the shelf ice. The leader had with him A. R. C. Butson, of the Falkland Dependencies Survey, and two members of the American expedition. This part of Antarctica had previously only been roughly surveyed, and to some extent incorrectly, from the air. Air transport was of service to the expedition in carrying supplies, but the most important exploration was in surface work.

Zoological Publications from Victoria University College, New Zealand

THE Department of Zoology of Victoria College, Wellington, New Zealand, has recently commenced a series of zoological publications in the form of individual pamphlets. The first six of these, No. 1, November 1, 1949–No. 6, May 31, 1950, are now to hand. They deal respectively with a new leech, new species of Ciliata, the Ctenophores, the Kirk collection of sponges, blood parasites of birds and new bugs (Hemiptera, Homoptera), and they vary in length from seven to twenty pages. The printing throughout is of a high order, and the illustrations, whether text-figures or plates, are quite adequate. The contents mainly deal with new species or the amplification of information concerning previously described forms, the exception being that dealing with sponges. This deals with the collection of sponge material, either whole specimens or slides, made by the late Prof. Kirk. While most of them are New Zealand species, some material is from outside sources, and of this some derives from the *Challenger* collection worked out by Prof. A. Dendy. It has made possible the establishment of a number of type specimens, holotype, syntype and merotype, as well as providing additional data on the localities of collection. One detail that calls for note is whether these publications will be regarded as establishing priority in nomenclature under the international rules, since they carry no indication of price or availability. In view of their excellence and usefulness, it would be a pity if they were considered to be invalid.

Experiments at the Grassland Research Station

A BULLETIN on "Experiments in Progress 1950" has been issued by the Grassland Research Station from Drayton, Stratford-on-Avon, Warwickshire. The Station took over the work and functions of the former Grassland Improvement Station, as from October 1, 1949 (see *Nature*, **163**, 477; 1949). New headquarters are being established at Hurley, Berkshire, but most of the experimental work listed in the bulletin has been carried out at Drayton. The experiments in progress include some welcome new features such as the investigation of the much-neglected fungus diseases of grassland. Some useful results obtained in earlier years are included in the publication. Yields of various grass strains for hay, grazing, silage and winter keep are compared and related with live-weight increases under stock. The relative yields of dry matter per acre under different methods of management are also tabulated. Many of the results are records which will become increasingly valuable as the years pass, but the bulletin indicates a large amount of useful long-term work soundly established.

Marine Radar for Harbour Use

A NOTE on harbour radar services, based on an article in the *Journal of the Franklin Institute*, was published in *Nature* of September 30, p. 544. Dr. J. B. Warren, of the Department of Physics, University of British Columbia, Vancouver, has provided some additional information. He states that an Admiralty Pattern 268 was installed at Halifax, Nova Scotia, in 1946, and in May 1949 the National Research Council of Canada installed an equipment on the centre of the Lions' Gate Bridge across the First Narrows in Vancouver Harbour. A description of this installation by W. J. Mooney appeared in *Harbour and Shipping* (Vancouver) of January 1950.

Briefly, the radar consists of a much modified 268 model, that is, 3-cm. wave-length with 1/4 microsecond pulse, operating into twin synchronized scanners placed on each side of the bridge, which give a beam-width of 1¼°, with a 12-in. plan-position display.

Reference List of Microfilms and Microcards

THE General Reference and Bibliography Division of the Library of Congress at Washington has issued a selected list of references on "Microfilms and Microcards: their Use in Research", compiled by Blanche P. McCrum (pp. 81; Washington, D.C.: Library of Congress Card Division, 1950; 55 cents). Its purpose is to indicate the sources from which may be gained an understanding of the history, development, spread and present utility of microfilms and microcards; but the list is not designed to appeal to the highly specialized interests of professional photographers and technicians, or to contribute to the literature of library administration in this field. References to developments in countries other than the United States are incidental rather than comprehensive, but the activities of the Royal Society Scientific Information Conference and of Aslib in this field are well covered. Part 1 of the list is limited essentially to books, monographs, bibliographies, catalogues and to periodicals devoted to the subject. Part 2 includes articles published in periodicals since January 1, 1946. A brief topical index and a key to symbols are included.

Library Binderies

THE Library Association has issued as its Pamphlet No. 3 a highly practical pamphlet on "Library Binderies" (pp. 40+4 plates; London: Library Association, 1950; 3*s*. 6*d*.; 2*s*. 9*d*. to members). Mr. James Cranshaw, formerly chief librarian, Kingston-upon-Hull, contributes a foreword, and both he and the author, Mr. R. F. Drewery, who succeeded him as chief librarian, emphasize the economies which the establishment of its own bindery can offer to a library, particularly in the saving of time, clerical, packing and accounting operations, and in the undertaking of minor operations in the way of labelling or numbering new books, and the provision of light-weight equipment for filing and pamphlet boxes and trays, book dummies and book troughs. The pamphlet then gives a layout for an establishment handling at a minimum ten thousand volumes a year and indicates the staff and equipment required and the conditions which have to be satisfied under the Factories Act and by trade union agreement or rules and regulations laid down by the local authority. Besides his practical advice on such matters and also on materials, tools and appliances, Mr. Drewery deals carefully with the question of economics and points out that, since labour costs are very high (forming approximately seventy-five per cent of the total expenditure), it is of first-rate importance to ensure that the team of work-people are kept fully occupied at their own jobs. Full realization of the advantages of a library bindery accordingly involves both the formation of a well-balanced team of work-people and careful planning and arrangement, so as to secure a continuous flow of work to the bindery which will keep the team continuously occupied. The pamphlet quotes Mr. Cranshaw's estimate at Eastbourne that at 1949 prices a small workshop, without any power-driven machines, could be set up for a capital cost of £1,100.

Grants for Field-work in Geology and Geophysics

THE Shell Petroleum Co., Ltd., is offering in 1951 a number of grants for postgraduate studentships in geology, at £375 a year, which will be in addition to those that have already been awarded. These studentships will be for work leading to the Ph.D. degree, and the only terms are that, first, the subject of the thesis, based on field-work, shall bear some relation, in stratigraphy and tectonics, to the conditions of oil occurrence; and, secondly, that by preference the student shall work elsewhere than at the university at which he graduated, a principle which, it is felt, might be more widely adopted. At the present time, Ph.D. work in geology is being done by students from the Universities of London, Bristol, Leeds and Newcastle in areas as widely separated as the Welsh and Scottish borders, the Pennines and western Ireland. There are, too, studentships in geophysics at the Imperial College of Science and Technology, London, and at Cambridge; but with the growth of interest in this subject, as, for example, at Birmingham, it is hoped to spread these studentships more widely. A second part of the Shell Petroleum Co.'s plan consists in encouraging geological field-work by distributing £2,500 annually to the various university geological departments. The grants are made primarily with the object of assisting undergraduate students to undertake individual mapping during the course of their study when there is no other source of funds, or for individual field-mapping out of term.

International Welding Congress

AN International Welding Congress will be held in London and Oxford during July 14–21, 1951. The Congress will include the annual meeting of the International Institute of Welding and its various Commissions, and is being sponsored in Great Britain by the five British member societies, namely, the Institute of Welding, the British Welding Research Association, the British Acetylene Association, and the Welding Sections of the British Electrical and Allied Manufacturers' Association and of the Sheet and Strip Metal Users' Technical Association. The Congress will open in London on July 14 and will move on the following day to Oxford, where three colleges, Christ Church, Oriel and Somerville, will accommodate the visitors, who are expected to number between four and five hundred. Meetings of the fourteen technical commissions of the Institute will occupy the first two days, and in the second part of the week there will be three open sessions for the presentation and discussion of papers, the sessions being on the welding of wrought light alloys, welding of bridges and allied structures, and present trends in British welding. After July 21, facilities will be provided through the twenty branches of the Institute of Welding for members to visit works in all parts of Great Britain. The secretary of the Congress is the secretary-general of the International Institute of Welding, Mr. G. Parsloe, and the office of the organizing committee is at 2 Buckingham Palace Gardens, Buckingham Palace Road, London, S.W.1.

ERRATUM. In *Nature* of November 25, p. 898, the article entitled "Financial Incentives" had a footnote referring to a booklet "Organizing for Output" published by the British Institute of Management, 8 Hill Street, London, W.1. This reference should have been to "Wage Incentive Schemes" (pp. 26, price 2*s*. 6*d*.) published by the same body.

SCATTERING OF ELECTROMAGNETIC WAVES BY ATMOSPHERIC TURBULENCE

By Dr. E. C. S. MEGAW
Royal Naval Scientific Service

Stellar Scintillation and the Spectrum of Turbulence in the Free Atmosphere

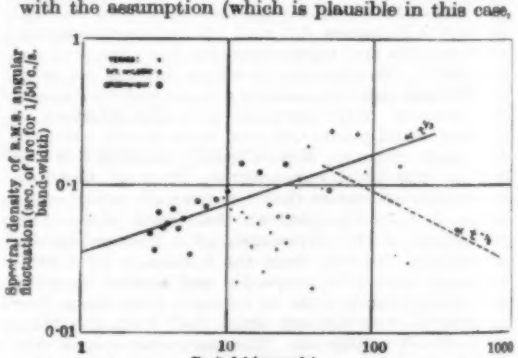
KNOWLEDGE of turbulent fluctuations in the atmosphere, whether of velocity or of other physical quantities, has not yet reached the point at which a representative statistical description can be discovered in the literature. Although aircraft measurements of temperature, humidity and acceleration fluctuations have been briefly reported[1,2] and are of great interest, knowledge of the average properties of turbulence in the free atmosphere is almost totally lacking. Recent developments in the statistical theory of turbulence, which may be expected to find application here, do not yet appear to have done so.

Against this background a description of atmospheric turbulence derived from the twinkling of the stars may be of interest. This was obtained in an investigation of the effect of fluctuations in refractive index on radio propagation beyond the horizon; this is referred to in the following communication[3]. In an earlier unpublished study of the scintillation observed in centimetre-wave transmission over unobstructed paths, it had been shown by J. M. C. Scott[4] that the radio observations were consistent in order of magnitude with the measure of refractive index fluctuation indicated by text-book descriptions of astronomical scintillation. My treatment of the astronomical case is essentially an elaboration of Scott's discussion to provide approximate numerical values, taking account of the spectrum of the fluctuations and of the variation of mean air density with height.

From the theoretical aspect a fundamental difficulty of atmospheric turbulence, viewed on a large scale, is that it has multiple energy inputs. On the largest scale one may regard turbulent dissipation in the free atmosphere as a partial sink for energy on its way from the equatorial boiler to the polar condensers; but locally variable convection is an inevitable complication, as is the lack of statistical homogeneity through the surface layer. The approach throughout the present work has been in terms of the statistical simplicity which it was thought might obtain in the free atmosphere, noting that changes in intensity and spectrum through the surface layer would, on the average, probably have little effect on stellar scintillation, and that these changes might to some extent compensate one another in the radio scattering problem.

The literature of stellar scintillation is considerable, but is widely scattered and evidently not well known; the recent controversy on the subject in *Nature*[5] would otherwise scarcely have been possible. There seems to have been no comprehensive discussion since the Pernter-Exner monograph[6] appeared early in the century, and the literature is not entirely free from contradictions and confusions. But the weight of evidence accumulated over the past hundred years leaves no doubt of the correctness of Newton's opinion that the twinkling of the stars is caused by variable atmospheric refraction. Some representative references have been given in the recent discussion, particularly by Minnaert[7]. As with the meteorological observations, satisfactory measuring technique (at least for brightness fluctuations) has only recently become available; but in this case the information does refer, in an integral form, to the whole thickness of the atmosphere. The fact that the phenomena of astronomical 'seeing' (in the narrow sense) are known to vary with time and place, and to show some correlation with surface weather, might be thought to suggest that the effects measured are wholly produced in the surface layer. There is much evidence to show that this is not so[8,9]. The faster fluctuations, which are obvious either to the naked eye or as blurring of telescope images, and which correspond to part of the small-eddy end of the spectrum, may, however, be much influenced by changes in turbulence near the ground.

The information on angular fluctuation of star images available from measurement of photographic star trails at several observatories was first examined. This information relates particularly to the *large-eddy* end of the spectrum. The accompanying graph shows fluctuation spectra obtained from Fourier analyses, kindly carried out by Mr. N. F. Barber at the Admiralty Research Laboratory, of measurements from Yerkes (40-in. refractor) and Mount Wilson (60-in. reflector)[10], and from Greenwich (Cookson 7½-in. floating zenith)[11]. The points for Greenwich are averages of corresponding components from two trails on the same plate, which may account for the smaller spread in this case; the others are from single representative trails (both a little above average in root-mean-square total fluctuation computed directly from the observations). The considerable spread in the points is scarcely surprising in view of the smallness of the sample which it was practicable to analyse; indeed, the measure of agreement between the three sets of observations is closer than might have been expected. The full line through the points is drawn with the slope ($\propto f^{1/3}$) which corresponds to the equilibrium spectrum derived by Kolmogoroff[12,13,14] for "locally isotropic" turbulence, with the assumption (which is plausible in this case,



Stellar scintillation spectra; angular fluctuation for zenith distance near 0°

Table 1. FLUCTUATION IN REFRACTIVE INDEX: FROM PHOTO-ELECTRIC RECORDS OF BRIGHTNESS FLUCTUATION

Source	Telescope aperture (in.)	Time constant (sec.)	R.M.S. brightness fluctuation (per cent)	Zenith distance	Remarks	Derived N'_s (m^{-1})
Kron (1949)	36	1·5	about 1	< 45°	Rough estimate from many records	1×10^{-7}
	12	"	about 1·5	"		1
Stebbins (1944)	4	about 0·01	18	about 40°		0·5
"	15	"	9			0·6
"	60	"	2·2			0·5
"	4	2	1·6			0·6
"	15	2	0·8			0·5
"	60	2	0·2		Doubtful reading	0·4
Kron (1950)	12	0·2	1·5,	0°	Extrapolated from records of 4 stars at Z.D. 8°–62°	0·5,

though it is probably not always correct) that the spectrum of refractive index fluctuation is the same as that of velocity fluctuation. This slope is at least an acceptable representation of the trend of the observations, though the slightly different slope ($\propto l^{1/3}$), corresponding to the Fourier transform of the exponential correlation coefficient introduced by Taylor[15] in the earlier form of the theory, represents an equally good fit. Although the probability that the ordinates at the long-period end of each individual spectrum are representative is rather small, each shows a tendency to decrease in amplitude in the region of $t = 50$ sec. or more; this is borne out by inspection of other trails of which a full analysis was not made. While the true average trend of the spectrum at the long-period end is quite uncertain, a decrease as $t^{-1/2}$ (dashed line) for $t > 50$ sec. is a possible representation in this region. If, as appears to be appropriate here, a mean upper-wind velocity of 15 m./sec. is assumed, these results suggest that the equilibrium spectrum extends to a largest eddy size of the order of 400 m. It can be shown that this corresponds to an integral scale of turbulence[15] of the order of 100 m. These figures appear reasonable; the first as being an order of magnitude smaller than the height of the equivalent uniform atmosphere, and the second in relation to meteorological estimates of mixing length[16]. The true values evidently may be larger, but can scarcely be much smaller.

Measurements of stellar brightness fluctuation have been made photographically[6], but have become really satisfactory only since the introduction of photo-electric photometry. The only published records of this kind appear to be some examples given by Stebbins[17], which were intended for demonstration rather than analysis. These have been supplemented by additional unpublished information kindly provided by Dr. G. E. Kron[18] of Lick Observatory. The recorded brightness fluctuations provide information particularly about the *small-eddy* end of the spectrum, and they are therefore greatly influenced by the telescope aperture and the time constant of the recording equipment. The dependence of the root-mean-square value of the recorded fluctuation on both these factors has been calculated; this is usually likely to dominate the 'natural' (viscosity) cut-off of the equilibrium spectrum, derived by Heisenberg[14].

Table 2. FLUCTUATION IN REFRACTIVE INDEX: FROM FOURIER ANALYSIS OF ANGULAR FLUCTUATION IN PHOTOGRAPHIC TRAILS

Source	R.M.S. angular fluctuation at Z.D. 0°	Derived N'_s ($m.^{-1}$)
Accompanying figure, assuming Kolmogoroff spectrum as represented by full line and terminated by viscosity at eddy size 0·1 m.	0·8,"	$1·1 \times 10^{-7}$

Note: The consistency of the results from Stebbins's observations in Table 1 implies that the 'smallest' eddy size of the equilibrium spectrum is not much greater than 0·1 m. An average figure of this order—perhaps rather less near the surface and rather more at great heights—is supported by much other evidence in the literature.

A measure of the total fluctuation in refractive index has been derived from each of the available records of brightness fluctuation, and also from the average observed spectrum of angular fluctuation as represented in the graph. The assumption is made that *all the observations can be accounted for by an average spectrum of turbulence which is a permanent characteristic of the atmosphere, and that this is, in fact, the Kolmogoroff spectrum.* The consistency of the derived values provides a test—in principle sensitive, but limited here mainly by the nature of the observations available—of the truth or falsity of this assumption. The results are given in Tables 1 and 2; the derived measure, N'_s, is the root-mean-square fluctuation in refractive index gradient, referred to ground-level.

In spite of the rather striking consistency of the final results, it would be premature to claim that the assumption italicized above has been justified until reasons for the spread (by about a factor 2) are definitely established. (The figure given in Table 2 for root-mean-square angular fluctuation, for example, is almost certainly larger than the true long-time average; this is probably nearer 0·5" from all the available evidence.)

The assumption may, however, fairly be taken as a starting point in the problem which gave rise to this study. The degree of statistical simplicity which this work has revealed also suggests that the principles of similarity, recently so fruitful in other fields, have an important contribution to make in atmospheric turbulence, in spite of all its complexities.

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Scattering of Short Radio Waves by Atmospheric Turbulence

Numerous measurements during the past fifteen years have led to the belief that the theory of diffraction around the earth gives a correct description of the propagation of radio waves beyond the horizon, provided (*a*) the waves are short enough to be unaffected by the ionosphere, and (*b*) the distribution of mean refractive index gradient is sufficiently like that of the so-called 'standard' atmosphere to be represented in the theory by a small modification of the earth's radius. From experiments carried out a few years ago it appeared first probable[1], and later almost certain[2], that this conclusion was sometimes false when the distance was great enough in relation to the wave-length and aerial heights used. In these experiments field-strengths too large to be accounted for by current theory were observed. It was suggested by Pekeris[3] that their cause might be a scattering process in the atmosphere.

I reached the conclusion that, in spite of apparent discrepancies, these 'anomalous' observations had a common cause, not in any exceptional condition of the atmosphere but in its permanently turbulent state and in the consequent fluctuation of refractive index. A rough calculation was made (towards the end of 1948) of the field-strength to be expected beyond the horizon from atmospheric scattering alone, with results in good agreement with those reported in ref. 1. This much-simplified treatment indicated that the scattered field-strength varied as the $-3/2$ power of the distance and was independent of wave-length. Afterwards the theory was extended to remove some of its more obvious deficiencies; a considerable study was made of available information relating to turbulence and to fluctuations of refractive index; and an experimental investigation was carried out with results outlined below.

The three-dimensional fluctuations of refractive index, assumed statistically stationary, are supposed analysed into a spectrum of triply periodic components. For a small interval of the spectrum a representative 'Fourier element' (a half-space-wave-length cube) is considered, and the field scattered by this element evaluated for the most favourable phasing in the three co-ordinates. It has been assumed—with little justification beyond plausible analogy—that the appropriate space integration, with random phasing between Fourier elements, gives the correct contribution to the scattered field. As it stands, this treatment leaves some doubt about the correctness of the final numerical coefficients; but it confers the great advantage of retaining the constants specifying a finite spectrum in the expression for the scattered field, after the final integration over the spectrum.

The result of this analysis, in a form applicable to wave-lengths from a few decimetres to about 2 cm., is outlined here. In general, when the distance is great enough for the diffracted field to be negligible, the ratio of the root-mean-square field-strength to the free-space field-strength at the same distance may be written

$$E/E_0 = Q\,N'\,K, \qquad (1)$$

where Q is a function of the geometry, N' is the root-mean-square fluctuation of refractive index *gradient* in an appropriate interval of height, and K is a function of the spectrum of turbulence. Units are: Q and K in m.$^{1/2}$; N' in m.$^{-1}$; all lengths in metres. A great simplification of the results is effected by restricting the discussion to small angles (beam-widths and scattering angles), and this appears always permissible in the wave-length range stated. The geometrical factor is then given by

$$Q = (2/\pi^3)\,L^{-1/2}\,R\,\{1 - (d_0/L)\}^{-1}\,F\,(p, b), \qquad (2)$$

where p is $2\theta/\alpha$ and b is φ/θ; L is distance and R is effective earth radius taking account of mean refraction which is assumed near 'standard'; d_0 is horizon distance for given transmitter and receiver aerial heights which, if dissimilar, must be small enough to make $d_0 \ll L$; θ, φ are elevation and azimuth semi-beam-widths; if not the same at transmitter and receiver, it is permissible to take the geometric mean of the two θ's, provided they are of the same order, and the lesser of the two φ's; α is angle between tangents to the earth from transmitter and receiver aerials, in the vertical plane through the aerials, which is equal to $(L/R)\,\{1 - (d_0/L)\}$.

Here F is a numerical factor given by a rather complicated volume integral in which a dependence on wave-length, atmospheric attenuation, and the variation of intensity and spectrum of turbulence with height, may all be ignored *for the stated wave-length range* without serious error. A few values of F have been obtained numerically:

p	0·1	0·3	1	3	10	30	100	
$b = \frac{1}{4}$	0·014	0·059	0·21	0·46	0·72	0·85	0·88	$F\,(p, b)$
$b = 1$	0·028	0·12	0·37	0·66	0·83	0·88	0·88	
$b = 4$	0·055	0·19	0·46	0·72	0·85	0·88	0·89	

For small distances—usually less than 100 miles—the difference of F from unity, and probably also the variation of N' with height, are not of much importance in view of other approximations. At larger distances both lead to an increasingly rapid decline in scattered field; to calculate this a known or assumed form for $N'(h)$, and hence $N'(L)$, must be introduced. The appropriate value of height h, which in any particular case can be obtained by straightforward but tedious numerical integration, is given roughly by

$$h = (L^2/2)\,[(1/4R)\,\{1 - (d_0/L)\}^2 + 0{\cdot}8 \times 10^{-6}\theta], \qquad (3)$$

in which the first term is simply the height of the earth-tangent intersection for small or equal aerial heights; this is a lower limit, since the contribution to the scattered field from below the horizon can be shown to be negligible.

Since the spectrum of atmospheric turbulence, discussed in the previous communication[4], covers a large range of eddy sizes (of order 10^3), the polar function of an elementary scatterer cannot be described by a single simple expression over the whole range of the spectrum and of position in space. In (at least) the conditions assumed here, the transition between the regions of validity of two simple forms of scattering function can be represented without serious error as an additional form of spectral cut-off. With the spectrum of ref. 4, assuming that changes with height may on the average be ignored, the factor K in equation (1) becomes

$$K = 2\,a_z^{2/3}\,a_\gamma^{-1/3} = 2\,a_z^{2/3}\,[(2L/R)\,\{1 - (d_0/L)\}]^{1/6}\,\lambda^{-1/3}, \qquad (4)$$

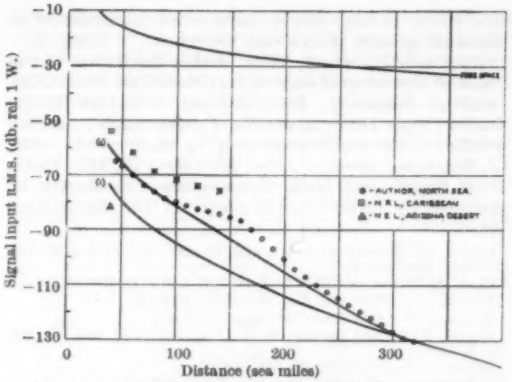
where a_z is the 'smallest' eddy size of the equilibrium spectrum (taken as 0·1 m.); $a_\gamma \equiv \lambda/2\alpha$ is the eddy size at which the cut-off representing the change in scattering function sets in; and λ is the radio wave-

length, which (in the present approximation) makes its only appearance here. It should be noted that $a_s \ll a_1$, where a_1 is the 'largest' eddy size of the equilibrium spectrum (order of 400 m.), is implicit in the evaluation of the product $N'K$, and so in equation 4. If the root-mean-square fluctuation in *refractive index* is N, in units of 10^{-6}, $N \approx 3 \cdot s$, N' for the spectrum assumed, a result which depends appreciably on the (uncertain) termination of the large-eddy end of the spectrum.

An independent theoretical solution of this problem has recently been published by Booker and Gordon[4]. Their analysis, though starting from a different and essentially more rigorous point of view[4] and leading to expressions superficially unlike those given above, has much in common with mine. The conclusions reached show some striking differences; these, however, chiefly reflect differences in the sources of information on atmospheric turbulence initially relied upon in the two treatments rather than the differences in analysis. Corresponding expressions for the scattered field can, in fact, be shown to be formally identical; but Booker and Gordon's results gain their simplicity from the use of an infinite spectrum—represented by an exponential correlation coefficient of unrestricted argument, which calls for some caution in application—and this precludes a straightforward comparison of numerical coefficients. With plausible limits to the spectrum, comparison in the long-range radio case suggests that my numerical coefficient is too small by a factor near 2. The material difference, however, apart from refinements and questions of generality, is that Booker and Gordon's final comparison of theory and observation appears to imply an integral scale of turbulence at heights up to about a kilometre, which is about 1,000 times too small for that height.

The results of field-strength measurements made at 10 cm. wave-length during six passages across the northern North Sea in the summer of 1949 are shown in the accompanying figure; these extended over a period of two and a half weeks in which a wide variety of weather conditions was encountered. Those parts of the original signal records which appeared from characteristic changes in the form of the record, supplemented by meteorological evidence, to be definitely influenced by super-refraction—which occurred to a marked degree part of the time—have been omitted. The circled points lie on the smooth curve considered to represent the remaining observations best; 90 per cent of them were within ± 5 db. of the curve. The observations of ref. 1 are represented by the square points. These define the lower envelope of a composite plot, Fig. 10 of ref. 1, of observations on 9·1-cm. wave-length in the Caribbean Sea; it is considered that this curve approximates to the condition of negligible super-refraction. The single triangle point at 40 miles represents daylight observations in the Arizona Desert[3] at the same wave-length, for small aerial heights. The discrepancy between the three sets of observations is reduced if allowance is made for instrumental differences; it is further reduced if allowance is made, according to any plausible assumption, for the differences in specific humidity between the three areas.

The full curve 1 is obtained from the theory, using the fluctuation spectrum (assumed independent of height apart from a change in root-mean-square value proportional to the gradient of air density) derived from the astronomical observations; an allowance for



Scattered field at 10 cm. wave-length: comparison of observed and calculated results.
Curves calculated; points observed

the greater refractive index of water vapour at radio-frequencies is included. As expected, this curve agrees best with the observations at great distances, that is, when the scattering is solely from large heights in the atmosphere.

In calculating the theoretical curve 2, the root-mean-square value of the fluctuating refractive index gradient near the surface has been increased to a larger value derived from radio (free-space) scintillation measurements; the corresponding value of N (root-mean-square) is near that adopted in ref. 5. The relative increase is assumed to decline with increasing height at a rate corresponding to the fall in average water-vapour content of the atmosphere.

The similarity of slope of observed and calculated curves, particularly between 50 and 100 miles, is probably the most definite evidence of the adequacy of the theory; the close numerical agreement with curve 2 at the largest and smallest distances must be considered accidental. The 'hump' in the observed curve near 150 miles (at which distance the most important heights are in the cloud layer, about 1–2 km.) is thought to represent a real difference between the simple assumptions of the theory and the actual distribution of turbulence and refractive index. Perhaps the most significant outcome is the apparent confirmation of the conjecture that turbulent scattering, at least of centimetre waves, represents a permanent, if slightly variable, modification of normal propagation beyond the horizon.

In regard to dependence on wave-length, the observations on 3·2-cm. wave-length in ref. 2 indicate, perhaps not conclusively, a considerably faster increase with decreasing wave-length than that indicated by equation (4). The problem of scattering at metre wave-lengths, which is theoretically more difficult, and may well show less clear-cut characteristics in practice, has not yet been fully examined. There is some indication that, allowing for the decrease in aerial directivity which commonly accompanies an increase in wave-length, the root-mean-square scattered field, expressed as a fraction of the free-space field, does not vary widely over the whole wave-length range in which it is observable. The general similarity of unexpectedly high field-strengths recently reported[7] at television wave-lengths and those recorded in the accompanying figure may not be entirely fortuitous. It is, indeed, probable that turbulent scattering, as one factor among several, plays an important part in determining the signals

received from high-power metre-wave transmitters at distances greater than about 100 miles. [Aug. 8.

Note added in proof. For further discussion of the value of the integral scale of turbulence see Proc. IXth General Assembly, International Scientific Radio Union (Sept. 1950), in course of publication; and for a different conclusion regarding the smallest eddy size, cf. Batchelor, *Quart. J. Roy. Met. Soc.*, **76**, 133 (1950). It is hoped that both these issues will shortly be clarified by direct measurement of the fluctuation spectrum.

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DETECTION OF FREE HYDROXYL RADICALS BY HYDROXYLATION OF AROMATIC COMPOUNDS

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THE existence of free hydroxyl radicals in the gaseous phase has been definitely established by the study of absorption spectra[1]. Haber and Willstätter[2a] and Franck and Haber[2b] introduced hydroxyl radicals as intermediates in the mechanism of reactions in aqueous systems, particularly in the case of oxidation processes. Hydrogen peroxide to which ferrous salt is added (Fenton's reagent) was employed long before it was realized that its action was due to the formation of free hydroxyl radicals which, according to Haber and Weiss[3], are formed by the reaction:

$$H_2O_2 + Fe^{2+} \longrightarrow OH + OH^- + Fe^{3+}. \quad (1)$$

The presence of free hydroxyl radicals in this reaction has been confirmed by M. G. Evans *et al.*[4a], who were able to show that this reagent can initiate the polymerization of suitable monomers. This is a powerful method which can be used generally for the detection of various kinds of free radicals[4,5].

It is known also that the hydrogen-peroxide/ferrous salt reagent is capable of hydroxylating aromatic compounds with the formation of phenolic products[6]. This reaction can be carried out under remarkably mild conditions, under which the normally inert aromatic compounds, for example, benzene, are not otherwise attacked. We have been able to show that this reaction may be used for the *specific* detection of free hydroxyl radicals in different reactions. Using benzene as the simplest case, the action of hydroxyl radicals leads to the formation of free phenyl radicals and phenol according to:

$$C_6H_6 + \cdot OH \longrightarrow C_6H_5\cdot + H_2O \quad (2)$$

$$C_6H_5\cdot + \cdot OH \longrightarrow C_6H_5OH \quad (3)$$

and of diphenyl:

$$2\,C_6H_5\cdot \longrightarrow (C_6H_5)_2. \quad (4)$$

The formation of diphenyl[4b] confirms the free radical nature of this reaction, as shown also by Merz and Waters[4d]. Using this method, we have established the presence of free hydroxyl radicals in the action of ionizing radiations (X- and gamma-rays, neutrons, alpha-particles) on dilute aqueous solutions[4b,7], where the radicals are formed according to the net process[8]:

$$H_2O \longrightarrow H\cdot + \cdot OH \quad (5)$$

The phenol and diphenyl formed when water is irradiated in the presence of benzene can be isolated under suitable conditions, or detected by various colour reactions. The same type of reaction may be carried out using other aromatic compounds as substrates, such as benzoic acid, nitrobenzene, etc.[6,7,9].

We have also applied this method to the study of photochemical reactions in aqueous solutions, where hydroxyl radicals are formed by the photochemical decomposition of water in the presence of ions such as uranyl or ceric[9] according to:

$$UO_2^{2+}.H_2O + h\nu \longrightarrow (UO_2^{2+}\cdot H_2O)^* \longrightarrow UO_2^{+} + OH + H^+ \quad (6)$$

and

$$Ce^{4+}.OH^- + h\nu \longrightarrow (Ce^{4+}.OH^-)^* \longrightarrow Ce^{3+} + OH. \quad (7)$$

Previously, it has been shown[4b] that in the case of ceric salts molecular oxygen is given off. With the polymerization technique, the occurrence of free radicals was established by Evans and Uri[4b] in the case of the photochemical decomposition of water by ferric ions, and by Melville and Whyte[4c] using uranyl salts. In our experiments[10], the formation of phenolic compounds could be shown even on irradiating, with visible light, solutions containing ceric or uranyl salts and an aromatic compound[4d].

Thus, in such widely different reactions as the catalytic decomposition of hydrogen peroxide, the action of ionizing radiations on water, the photochemical decomposition of water by certain metal ions involving single-electron transfer processes, and apparently also some well-defined cases of electrolytic oxidations[11], all the available evidence is in agreement with, and can be explained most easily by, the assumption of a mechanism involving free hydroxyl radicals.

This comparison is based on the qualitative analogy between the various reaction mechanisms. The problem can be taken a step further if in addition a quantitative study is made of the hydroxylation of different monosubstituted benzene derivatives, where the ratio of the isomers formed under various conditions is accurately determined. Agreement in the proportions of the hydroxylated *ortho*-, *meta*- and *para*-isomers should give a more rigorous test as to whether the same mechanism is operating in two different cases. Applying this criterion it might be possible to approach the problem of certain processes *in vivo*, which may involve hydroxyl radicals or radicals of a similar nature as intermediates.

It has been known for about a hundred years that some aromatic compounds are hydroxylated in the human and animal body[12]. Dakin[12] paid particular attention to some similarities between these products and those obtained by the action of hydrogen peroxide. Haber and Willstätter[2a] gave the first discussion of the possibilities of free-radical reactions *in vivo*. It is possible that aromatic hydroxylation is capable of contributing to the elucidation of this problem. However, it would be very rash to conclude simply on the basis of the remarkable ease with which these reactions occur *in vitro* and from some qualitative similarity of the reaction products to those of the corresponding metabolic processes, that identical reaction mechanisms are involved in both cases.

Different reaction paths, involving enzymes, might easily yield similar products without the intervention of radicals. It is here that the quantitative approach might help.

In general, the positions in which a further substituent is introduced into an already substituted benzene ring and the relative proportions of the possible isomers formed depend on the existing substituent or substituents and the nature of the substituting agent. There is no general agreement as to whether free-radical substituting agents should be subject to a directive action. If the directive action does not operate, all possible isomers should be formed in the statistically appropriate ratio; that is, in the case of a monosubstituted benzene derivative, further substitution should yield one *para*-isomer for every two *ortho* and two *meta* formed, independently of the nature of the existing substituent. It has also been suggested[14] that in free-radical substitution the quinonoid structure of the transition state is of such importance that substitution will always be preponderantly *para* with some *ortho*. It is agreed, however, that free-radical substitution should be quantitatively different from substitution by ionic agents, and thus a quantitative investigation of the substitution ratios by reagents known to yield free hydroxyl radicals is therefore of considerable interest.

The hydrogen-peroxide/ferrous salt reagent is not very well suited for quantitative investigations of this nature. However, we have used this reagent as the basis of a quantitative comparison with the action of ionizing radiations, and using nitrobenzene as the substrate we have shown that, within experimental error, the ratios of the three isomeric nitrophenols formed in either of these processes are practically identical[9c,7b]. In this case, where the existing substituent is the *meta*-directing NO_2-group, all three possible isomers are formed in approximately equal quantities, the formation of the *para*-isomer thus being favoured.

We have now used the method of producing hydroxyl radicals by X-rays (which is particularly suited for quantitative work) to investigate the ratio of isomers in the substitution of benzoic acid and phenol. Benzoic acid gave all three isomers in the ratio $p : o : m$ approximately $10 : 5 : 2$[7a,7d], whereas in the case of phenol no *meta*-isomer was found, the ratio $p : o$ being approximately $2 : 1$ in neutral solution, and rising to approximately $8 : 1$ in acid or alkaline solutions[7c]. We have also investigated in some detail the effect of *p*H on the substitution ratios. All these results show that *para* substitution is generally favoured and that the ratio of the isomers depends on the primary substituent in a manner which is quantitatively different from other types of substitution.

To compare these results with experiments *in vivo*, reference can be made to the recent work of R. T. Williams *et al.*[15] on the metabolism of aromatic compounds. Some of their results and those of Lederer *et al.*[16] are not incompatible with the assumption of a free-radical mechanism *in vivo*, but more experimental results are needed before any definite conclusions can be drawn.

Even in those cases where there is quantitative agreement in the substitution ratios, it is by no means proved that hydroxyl radicals are actually operative *in vivo*. However, it would necessitate the assumption of transition states, which are effectively free radical in nature, though they may be formed on the surface of an enzyme, in the adsorbed state. There is also the possibility that more than one type of mechanism may operate at various biological sites, and while dihydro-diol formation[17] may be the mode of action in one mechanism, at other sites free-radical substitution may operate. Preferential conjugation, rather than hydroxylation of intermediates, must also be considered before further conclusions can be drawn in this important problem.

[1] (*a*) Bonhoeffer and Reichardt, *Z. phys. Chem.*, **139**, 75 (1928). (*b*) Oldenberg and Rieke, *J. Chem. Phys.*, **6**, 169 (1938).
[2] (*a*) Haber and Willstätter, *Ber.*, **64**, 2844 (1931). (*b*) Franck and Haber, *Sitzb. Preuss. Akad. Wiss.*, 250 (1931).
[3] Haber and Weiss, *Proc. Roy. Soc.*, A, **147**, 332 (1934).
[4] (*a*) Baxendale, Evans and Park, *Trans. Farad. Soc.*, **42**, 155 (1946). (*b*) Evans and Uri, Sympos. Photochemistry of Dyestuffs, Soc. Dyers and Colorists, 125 (1939). (*c*) Melville and Whyte, *ibid.*, 119. (*d*) Weiss, *ibid.*, 135.
[5] Dainton, *Nature*, **160**, 268 (1947).
[6] (*a*) Cross, Bevan and Heiberg, *Ber.*, **33**, 2015 (1900). (*b*) Stein and Weiss, *Nature*, **161**, 650 (1948). (*c*) Loebl, Stein and Weiss, *J. Chem. Soc.*, 2074 (1949). (*d*) Merz and Waters, *ibid.*, 2474 (1949).
[7] (*a*) Stein and Weiss, *J. Chem. Soc.*, 3245, 3254 (1949). (*b*) Loebl, Stein and Weiss, *ibid.*, 2704 (1950). (*c*) Stein and Weiss (to be published). (*d*) Loebl, Stein and Weiss, *J. Chem. Soc.* (in the press). (*e*) Day and Stein, *Nature*, **164**, 671 (1949).
[8] Weiss, *Nature*, **153**, 748 (1944); **157**, 584 (1946); *Trans. Farad. Soc.*, **43**, 314 (1947); *Brit. J. Radiology*, Supp. 1, 56 (1947).
[9] (*a*) Carter and Weiss, *Proc. Roy. Soc.*, A, **174**, 351 (1940). (*b*) Weiss and Porret, *Nature*, **139**, 1019 (1937).
[10] Unpublished experiments of T. Rigg.
[11] (*a*) Walker and Weiss, *Trans. Farad. Soc.*, **31**, 1011 (1935). (*b*) Hückel, *FIAT* Reports, Theoretical Organic Chemistry.
[12] Williams, "Detoxication Mechanisms" (1947).
[13] For example, Dakin, *J. Biol. Chem.*, **4**, 235 (1908).
[14] Wheland, *J. Amer. Chem. Soc.*, **64**, 900 (1942).
[15] Smith and Williams, *Biochem. J.*, **46**, 243 (1950), and references given there.
[16] Lederer, *J. Chem. Soc.*, 2115 (1949).
[17] Cook, *J. Chem. Soc.*, 1210 (1950). cf. Boyland, Biochem. Soc. Sympos. No. 5, 40 (1950).

CLASSIFICATION, TRANSMISSION AND DETECTION OF ODOURS

A SYMPOSIUM on olfaction provided the main item of interest at a special general meeting of the Society of Cosmetic Chemists, held in the main hall of the Chelsea Polytechnic, Manresa Road, London, S.W.3, on the evening of October 20.

The subject was introduced by Dr. William McCartney, of the Iodine Educational Bureau. Speaking on "The Mysteries of Olfaction", Dr. McCartney referred at some length and in very interesting detail to the literary and historical background of the subject, which as a basis for study certainly dates back to the ancients' preoccupation with incense and was specifically discussed by Theophrastus in his essay on odours. Throughout the Middle Ages interest in olfaction was sustained and later assumed considerable importance in the writings of Montaigne and of Robert Boyle, who not only enunciated the celebrated law that bears his name and isolated methyl alcohol and acetone, but also apparently detected the odour of musk emanating from a decomposed heap of manure.

Passing in brief review Hippolyte Cloquet's "Osphrésiologie" (1815 and 1921), Dr. McCartney referred successively to the works of Saveliev, Zwaardemaker, Henning, Heyninx, Parker, Régismanset, Von Skramlik and others, and then to the more recent contributions to the subject made by Kenneth, Moncrieff, Sagarin, McCord, Witheridge, Le Magnen, Duncan and Cheesman. Reference was also made to specialized works dealing with the sense of smell in animals, culminating in recent years with the valuable paper published by Walter. Olfactory perception among primitive peoples, the blind, and

specialists such as tea-tasters and wine-tasters, represented a further approach.

Reference was doubtless intended to the investigations of Duncan and Cheesman in the observation that "in the domain of psycho-physiology, an up-to-date examination of the application of the Weber–Fechner law to the sense of smell seems desirable", in that the technique of the two workers mentioned is based on the type of statistical approach directly derived from that law, which may be briefly set down in the following terms: "sensation is proportional to the logarithm of the stimulus, it being necessary to increase the stimulus geometrically in order to increase the sensation arithmetically"[1].

The difficulties confronting the would-be measurer of odour intensities and the scientific investigator of odour qualities were mentioned by Dr. McCartney, who made no attempt to belittle them but nevertheless stopped short of the somewhat complacent references of Parry and Sagarin to what the latter has termed "the inherent invalidity of all systems of odour classification".

Dr. McCartney considered that vibratory or wave theories of odour transmission have "overwhelming evidence against them". In regard to the various theories held by Beck and Miles, Cheesman and Duncan, Duranton, Guillot, Le Magnen, McCord and Witheridge, Moncrieff and others, he felt it impossible that "all these theories, which are to some extent mutually exclusive, can stand the test of time". "In any case," he concluded, "the modern theories and the experimental results on which they are based provide ample material for much fruitful discussion. No scientific subject involves mysteries more profound than those of olfaction. And certainly no subject is more fascinating and absorbing."

The next speaker, Dr. G. H. Cheesman, confined most of his remarks to a description of the type of work now being carried out on odour classification at the University of Reading. After referring to the more obvious difficulties of devising a reliably objective, quantitative approach to the inherently subjective field of olfaction, he went on to emphasize the comparatively straightforward character of 'threshold' measurements, which can not only be determined with the minimum subjective interference, but are also susceptible to the psychological technique of factor analysis[2].

Dr. D. R. Duncan, a former collaborator in this work with Dr. Cheesman, and the co-inventor with him of an improvement on the conventional Fair–Wells osmoscope, then described in some detail the main features of the 'odour fatigue' method whereby threshold values are determined. Dr. Duncan had previously read a paper entitled "The Perception and Measurement of Odour" before the British Esperanto Scientific Association[3].

A somewhat radical departure from the domain of determining threshold values was represented by the views of the next speaker, Dr. R. R. Matalon, of the Department of Colloid Science, University of Cambridge. Dr. Matalon harked back to the work of Prof. Henri Devaux, which had been briefly referred to in the introductory talk by Dr. McCartney. Devaux's experiments showed the phenomenon of odour transmission in visible form, as a surface-active effect on a bath of mercury covered with talc, the talc-dusted surface showing characteristic disturbance when subjected to the proximity of rose petals and other odorants[4]. Dr. Matalon went on to connect this fundamental work with the probable action of odorants on the cells or 'receptors' of the olfactory nerve system. It seemed to him that further basic research on these lines is an essential prerequisite to the objective study of odours and any acceptable attempt at scientific classification.

The final statement submitted to the symposium was a written contribution by Dr. J. H. Kenneth, of Edinburgh, whose "Osmics: The Science of Smell" was published in its complete form in 1924.

The discussion started off somewhat slowly, but soon assumed a more interesting character—elucidation being not unmixed with critical comment and the statement of controversial opinions. Some speakers supported, for example, the vibrational theory of odour transmission, while others appeared even to doubt the validity of what is perhaps rather ambiguously described as 'odour fatigue'. The general feeling seemed to be in favour, on one hand, of the continuance of such work as is currently being undertaken by the University of Reading and, on the other, of devising a new type of approach to the subject that would logically continue the line of experimentation first indicated by Devaux. Among those who joined in the discussion were Mr. Jack Pickthall, Mr. E. S. Maurer, Mr. D. W. A. Waite, Dr. A. W. Middleton, Mr. Frank Atkins, Mr. James Bather and the Society's chairman, Mr. F. V. Wells.

[1] See R. W. Moncrieff's "The Chemical Senses", pp. 53–55.
[2] See *Soap, Perfumery and Cosmetics*, 697 (July 1949).
[3] See *Nature*, March 11, p. 394.
[4] See R. W. Moncrieff's "The Chemistry of Perfumery Materials" p. 279.

RHEOLOGY AND THE CONSTITUTION OF MATERIALS

ANNUAL CONFERENCE OF THE BRITISH SOCIETY OF RHEOLOGY

THE annual conference of the British Society of Rheology was held at St. Patrick's Hall, University of Reading, during September 28–30. The general title of the conference was "Rheology and the Constitution of Materials", and the papers on this subject stimulated discussions ranging over a very wide range of materials from metals to polymer solutions. At the annual general meeting of the Society, the change of name from the British Rheologists' Club to the British Society of Rheology was sanctioned, a change which reflects the important part which the Society is playing both as a link between men of science from different fields of work in Great Britain and as a prime mover in the formation of the International Union of Rheology. The officers for 1950–51 are: *President*, Dr. G. W. Scott Blair; *Honorary Secretary*, Dr. E. W. J. Mardles; *Honorary Treasurer*, Dr. J. G. Oldroyd; *Honorary Editor*, Dr. V. W. G. Harrison.

The presidential address, given on September 28 by Dr. G. W. Scott Blair, was on "The Rheology of Uterine Cervical Secretions". Dr. Scott Blair outlined the work which he and his colleagues have done on the relation of the properties of these secretions to the time of œstrus and the course of pregnancy for cows, and also similar studies made in conjunction with Mr. A. F. Clift for women. An emptying and filling capillary micro-viscometer was designed for this work which enabled measurements to be made with only 0·06 ml. of secretion. The original instrument, while giving very satisfactory results, was very tedious to use, and a new instrument giving

automatic methods will shortly be brought into use. A small-scale rotational viscometer was also described; it has been found that the shear in this instrument destroys the rheological properties most nearly related to the changes in hormone condition of the animals.

The presidential address was followed by a paper by L. Grunberg and Dr. A. H. Nissan on "Viscosity, Constitution and Structure", with A. G. Ward in the chair. Mr. Grunberg outlined the history of the formula $\eta = A \exp(B/T)$ and its many variants. After considering the classification of liquids into "normal or dispersion liquids", "polar associated liquids", "ionic liquids" and "metallic liquids", the main part of the paper was concerned with the "normal liquids". In comparing two liquids, the need for choosing suitable conditions in each case was emphasized, and the choice of temperatures at which the vapour pressures are equal was justified on the basis of theoretical arguments. A simpler method involves the comparison of viscosities at equal T/T_B, where T is the temperature for the viscosity measurement and T_B is the boiling point under atmospheric pressure. The results of the application of this method to hydrocarbons and their halogen derivatives were given together with conclusions derived from the study of $E_{visc.}/M$ $(=RB/M)$, where M is the molecular weight. The relation of the heat of mixing to viscosity data for regular solutions was also given. A short contribution was made to the discussion by Dr. A. Nisbet, outlining the theory of viscosity developed by Born and Green.

The opening session on September 29 consisted of a paper by W. G. Harland and W. A. Richardson on "Intrinsic Viscosity", J. G. Oldroyd being in the chair. Mr. Richardson showed how important an exact statement of the procedure followed can be in determining the intrinsic viscosity of solutions. He reviewed the many equations which have been developed to express the concentration dependence of the viscosity of polymer solutions, and also the terminology which has been suggested by different authors. Mr. Richardson expressed the firm opinion that it is better to ensure that the terms used—specific viscosity, intrinsic viscosity, reduced viscosity, etc.—should be those with the greatest chance of widespread adoption, rather than that an attempt should be made to impose a logical and rational new set of terms. Discussion following the paper showed general agreement with Mr. Richardson's views.

The paper by Dr. F. R. Eirich (Brooklyn) on "Flow Birefringence: Some Results on Concentrated Polymer Solutions" was, in Dr. Eirich's absence, read by Dr. E. W. J. Mardles. A. H. Nissan was in the chair. The relation of the intrinsic viscosity, sedimentation constant, translational diffusion constant and rotatory diffusion constant to molecular size, shape and molecular weight were first discussed. The need for two measurements of this type to enable molecular constants to be calculated was emphasized. Particular attention was given to the combination of the intrinsic viscosity with the rotatory diffusion constant, the latter being determined from the extinction angle, and to the relation of the results to molecular models for chain molecules.

In the afternoon a visit was paid to the National Institute for Research in Dairying, Shinfield, Reading. Dr. Scott Blair and his collaborators showed exhibits illustrating work on the rheology of cheese and butter, including the use of factorial analysis, and also the apparatus used for cervical secretions. A film was shown of the scientific study of cheese testing, and a general tour of the Institute was arranged.

The evening session was devoted to papers by Dr. L. R. G. Treloar on "The Structure and Properties of Rubber", and by H. H. Macey on "The Flow of Clay Systems". Dr. V. W. G. Harrison was in the chair. Dr. Treloar reviewed the theory of rubber-like elasticity and the crystallization of rubber, and illustrated his paper by convincing demonstrations of the various effects. Mr. Macey developed a quantitative theory of the plasticity of clay–water systems based on the forces of interaction between the clay particles. The interaction can be represented by repulsive forces which are compensated at the boundary by surface tension. The theory is able to give both a qualitative and quantitative interpretation of much of the experimental results for plastic clay. Considerable discussion followed Mr. Macey's paper concerning the nature of the water film between the clay particles, and whether any structure is imparted to the water by the forces from the clay particle surface over distances of more than a few molecular diameters.

The morning session of September 30 was devoted to the rheology of metals, with Prof. M. Reiner (Haifa Technical College) in the chair. The first paper, "The Role of Structural Discontinuities in Crystal Plasticity", was given by K. E. Puttick and M. W. Thring. Emphasis was placed on the importance of the rate of strain in measurements of yield of metal. The theories explaining yield in terms of dislocations were described and related to the experimental results. P. Feltham in his paper, "Trends in the Rheology of Metals at High Temperatures", stressed the dangers of approaching so complex a field from a narrow or mechanistic point of view. This was shown to lead to a divorce between 'pure' physical studies, suited to the academic worker, and 'applied' metallurgical studies which are left to the works laboratory. These considerations were illustrated by the inelastic behaviour of metals at high temperatures.

ALAN G. WARD

THE AGRICULTURAL AND HORTICULTURAL RESEARCH STATION, LONG ASHTON

ANNUAL REPORT FOR 1948

THE University of Bristol Agricultural and Horticultural Research Station, at Long Ashton, maintains its tradition of balanced practical research into problems of fruit growing and fruit products. In its annual report for 1948*, Dr. L. C. Luckwill reports that alpha-naphthalene acetic acid (5 p.p.m.), 2 : 4 dichlorophenoxy-acetic acid (1 p.p.m.) and indolyl-acetic acid (5 p.p.m.) all reduced fruit-set without causing damage to the foliage. These substances were applied as sprays shortly after the time of full blossom, and are designed to control superabundant fruiting, with subsequent abscission and loss of too much fruit. Dr. Luckwill has also studied the ecology of orchard cover crops, finding that rye-grass and clover mixtures tend to be replaced by creeping bent. The last-mentioned grass, however, is itself good for the purpose. C. Bould, J. A. H. Tol-

* University of Bristol. Annual Report of the Agricultural and Horticultural Research Station (the National Fruit and Cider Institute), Long Ashton, Bristol, 1948. Pp. 250. (Bristol: The University, 1949.)

hurst and R. M. Jarrett have investigated the effect of cover crops on fruit-tree nutrition, finding they had little effect on yield and girth increase, but lowered the nitrogen content of foliage, as compared with clean cultivation. C. Bould and J. Tolhurst bring further evidence that magnesium deficiency of fruit trees can be cured effectively by three or four foliage sprays of 2 per cent magnesium sulphate.

Prof. T. Wallace, director of Long Ashton Research Station, leads a group of workers on problems of plant nutrition, the present papers dealing mainly with the effects of farmyard manure on potatoes (with E. Catlow). E. J. Hewitt has an interesting paper on the resolution of factors in soil acidity, discussing the relative effects of aluminium and manganese toxicities. The same worker reports on iron deficiency and (with E. W. Jones) on molybdenum deficiency.

Dithiocarbamates, organic mercurials, substituted quinols and glyoxalidine derivatives have been suggested as alternatives to lime sulphur spray for the control of plant diseases. These are described in a short paper by R. W. Marsh; glyoxalidine seemed to give the most promising results. The proper healing of wounds is an important factor in plant pathology. S. H. Crowdy has studied the effect of organic chemicals on the process, and makes the suggestion that the inclusion of plant growth substance in wound dressings may have advantages. Some promising results on the control of brown rot of fruits by spraying with phenyl mercury chloride are reported by R. J. W. Byrde. The insecticide DDT has attained such popularity that it is natural to investigate the toxic properties of its analogues. This is being done by S. H. Bennett, H. Martin, A. Stringer and D. Woodcock, who issue a progress report.

A former director of the Station, Prof. B. T. P. Barker, contributes a paper on 'cider sickness', finding at least three different bacteria which can cause it. S. W. Challinor and L. F. Burroughs contribute two papers dealing with the role of nitrogen in fermentation, as part of a fundamental study of the chemistry of cider. As another part of the inquiry into cider-making processes, Margaret E. Kieser, A. Pollard and Audrey M. Stone report on the pectose activity of some apple varieties. The browning of processed pears can be avoided by quick processing, to attain temperatures which inactivate the enzymes (Dilys James and Alice Crang).

The research at Long Ashton is thus a welcome blend of fundamental and applied studies, and the report itself is informative. It is now edited by G. T. Spinks, who is to be congratulated on the maintenance of its high standard of production.

HIGH-TEMPERATURE ADIABATIC CALORIMETRIC MEASUREMENTS OF THE TRANSITION METALS

THE advantage of adiabatic calorimetry is that it allows specific heats to be measured directly over small temperature intervals and thus to obtain details of the variation of specific heat with temperature. Until recently, however, because of the difficulties of adequate thermal insulation at high temperatures, accurate work with adiabatic calorimeters was not possible at temperatures above 500° C. Further, the atomic heats of metals in the transition group, chromium to nickel, are of considerable theoretical interest; but reliable data at high temperatures exist for iron and nickel only. Two papers which appeared in the January issue of the *Canadian Journal of Research*[1] are therefore of considerable interest. In the first, a description is given by L. D. Armstrong of the construction of a new high-temperature adiabatic calorimeter which, by making use of special techniques developed previously for the measurement of thermal conductivities at high temperatures[2], can be used for making reliable determinations of specific heats up to 800° C.

The specimen, either a machined cone of solid metal or in the form of a powder or loose pieces packed firmly together, fits into a copper container, and the specimen and container are electrically heated. The surrounding copper radiation jacket, which is maintained at a temperature as close as possible to that of the container, is supported inside an electrical furnace which can be evacuated or filled with an inert gas. Temperatures are measured by thermocouples. The heat capacity of the empty calorimeter is determined by two series of measurements: one with the calorimeter empty; and the other with the calorimeter filled with the standard material, aluminium oxide in the form of synthetic sapphire, the specific heat of which is known accurately from the work of D. C. Ginnings and R. J. Corruccini[3]. It is claimed that specific heat values derived from a smoothed specific heat/temperature curve obtained from measurements made with the adiabatic calorimeter are correct to within 1 per cent.

In the second paper, measurements of the specific heats of chromium, manganese and cobalt made with the adiabatic calorimeter are described by L. D. Armstrong and H. Grayson-Smith. The chromium used was in the form of electrolytic flakes and was 99·9 per cent pure. The atomic heat of chromium is regular from 0 to 800° C., increasing steadily above 100° C. to values considerably in excess of the classical value $3R$. For manganese, in the form of electrolytic nodules and of 99·99 per cent purity but slightly discoloured by surface oxidation, the measurements show that the atomic heat is quite regular from 0 to 700° C. with values considerably greater than $3R$. At 717° C. the transition from the α- to the β-phase takes place sharply with a latent heat of 450 cal./mol. Supercooling occurs on cooling from the β- to the α-phase. Except for the interval 447–478° C., where cobalt changes its structure on heating from hexagonal close-packed to face-centred cubic, the atomic heat increases regularly. The heat of transition was estimated to be 60 cal./mol. approximately.

The experimental values are used for a theoretical discussion of the atomic heats of the transition metals, all of which are considerably greater than $3R$ at high temperatures. The excess specific heats of the ferromagnetic substances cobalt and nickel can be accounted for by the ferromagnetism and the conduction electrons. For chromium and manganese, after allowance is made for the conduction electrons, there is still a large residual excess specific heat indicating some unknown source of internal energy, attributed tentatively to antiferromagnetism. It is suggested that the two metals may have antiferromagnetic Curie points at temperatures above 800° C., but there is little evidence at present to support such a proposal.

[1] *Canadian J. Res.*, **28A**, 44 and 51 (1950).
[2] *Canadian J. Res.*, **25A**, 357 (1947).
[3] *J. Res. Nat. Bur. Standards*, **38**, 583 (1947).

LETTERS TO THE EDITORS

The Editors do not hold themselves responsible for opinions expressed by their correspondents. No notice is taken of anonymous communications

The Helium-3 Problem of the Atmosphere

In a communication in the *Zeitschrift für Naturforschung* of August 2, 1950, we have reported our attempts to prove the presence of tritium (H_3) in the atmosphere. The free atmospheric hydrogen, which is found in the air in a concentration of 5×10^{-7}, was burned with copper oxide out of the helium–neon fraction of an air-liquefaction plant. The water was electrolysed down to one-fortieth of its volume in order to enrich the tritium twenty times. The tritium-containing hydrogen of the enriched water was used to make ethane, the activity of which could easily be examined. According to our measurements, about one mole of elementary tritium is present in the whole dry atmosphere of the earth, or one atom of tritium in 10 cm.³ of air. Evidently this tritium has been produced by nuclear fission in the upper layers of the atmosphere, and did not take part in the atmospheric water circulation, but has remained elementary hydrogen.

Now it is possible to compare the amount of helium-3 found in the earth's atmosphere with that produced in the course of the earth's history by tritium, which has a half-life of 12 years, breaking down to helium-3. For this purpose it is necessary to have a precise total balance for the tritium. We have, indeed, determined the amount of tritium in the free hydrogen of dry air; but it is to be expected that a much higher amount of tritium will take part in the water circulation of the atmosphere. On one hand this amount can be estimated from kinetic relations and the distribution of the cosmic radiation intensity; on the other hand, the measurements of Willard F. Libby (Chicago) and A. V. Grosse (Temple University), published in *Time* of September 25, 1950, give the concentration of tritium in rain water as 10^{-18}, from which follows the interesting result that there is an enrichment of about 10,000 in atmospheric hydrogen compared with rain water.

If the intensity of the cosmic radiation has always been the same as now, and taking $3{\cdot}6 \times 10^9$ years for the age of the earth, there should be 2×10^7 atoms of helium-3 in 1 cm.³ of air under normal conditions, if only the tritium in the free hydrogen of the dry atmosphere is taken into account. In fact, there are $1{\cdot}5 \times 10^9$ atoms of helium-3 in 1 cm.³ of air. It is obvious that this estimated concentration of helium-3 is only one-eighth of that really found.

However, the cosmic radiation may have produced primarily as much helium-3 as tritium; furthermore, a large part of the tritium has certainly come into the water circulation. With an assumed mean annual rainfall of 18 cm., averaged over the whole earth, and a concentration of tritium of 10^{-18}, as measured by Libby and Grosse, the estimated figure for the concentration of helium-3 should rise to about 5×10^9 atoms/cm.³. In this case most of the helium-3 must have left the earth.

By measuring the concentration of tritium in several samples of C_2D_6 prepared from heavy water, we hope to get a fairly accurate value for the tritium content of the whole earth, which would form a necessary base for discussing the problem of the loss of helium-3 from the atmosphere.

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VOLKERT FALTINGS
Institut für physikalische Chemie,
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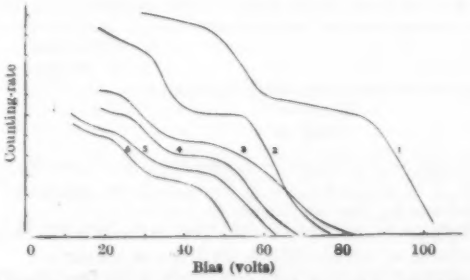
Sept. 30.

Alpha-Particle Scintillations in Organic Compounds

THE papers[1–7] previously published on organic luminescent materials do not enable a direct and reliable comparison of their conversion efficiencies (conversion of incident radiation energy into light energy) to be made. This is so because different types of photomultipliers have been used and not all the compounds have been investigated with similar radiation. Further, the results reported are not consistent. Most of the materials previously used have been re-investigated in this laboratory under the same conditions in each case and their efficiencies as scintillation counters assessed. The effective use of a fluorescent material depends on the spectral response of the photomultiplier and on the wavelength of the light emitted when a scintillation takes place. The results presented are therefore of additional interest, as *RCA* 931 type photomultipliers were used to obtain the major part of the information available in the literature, whereas the results given here were obtained with an *EMI* 5311 photomultiplier which has a different response. In addition, materials not so far reported on have been investigated.

Results obtained with α-particles seem to provide the most satisfactory basis of comparison. Integral bias curves have been obtained using α-particles (6·04 and 8·72 MeV.) from thorium C and C′. This procedure has the further advantage that thin crystals with an area of one or two square millimetres are adequate. The crystals chosen were as thin as possible, consistent with stopping all the α-particles. In this way, the β- and γ-ray background was kept to a minimum. Results were considered reliable only if the integral bias curve showed two α-particle groups indicating complete stoppage of the lower group, which could therefore be used for comparing the performances of the compounds.

To ensure that measurements of this type shall be reliable, it is necessary to pay particular attention to the purity of the samples[8]. As only a few small crystals were required, it was possible to obtain a good deal of purification by recrystallization from



Integral bias curves for organic phosphorus using α-particles from thorium C and C′: (1) anthracene; (2) naphthalene; (3) chrysene; (4) 1.4 diphenyl butadiene; (5) 2.2′ dinaphthyl; (6) stilbene

organic solvents. Repeated recrystallization enabled the purity to be checked by observing the pulse-heights obtained with crystals from successive samples; but, of course, one cannot be certain that all impurities have been eliminated.

The bias value, corresponding to the lower α-particle group obtained from an integral bias curve for each substance, has been used to sort the materials into the three groups shown in the accompanying table. The curves shown are integral bias curves for the substances tabulated in groups 1 and 2. The relative heights of these curves have no significance as each curve was obtained with a different counting-rate.

RELATIVE EFFICIENCIES OF ORGANIC PHOSPHORS

~ 50 volts	~ 30 volts	< 20 volts
Anthracene	Stilbene 2.2′ Dinaphthyl*	Pyrene Phenanthrene Acenaphthene
	Chrysene* 1 : 4 Diphenyl butadiene*	Fluorene Dibenzyl Anthranilic acid
	Naphthalene + ~ 1 per cent anthracene	Fluoranthene* 1 : 2 Benzanthracene Diphenyl Triphenyl methane

* Not previously reported.

The following points are of interest.

(1) When scintillations are caused by thorium-C α-particles, the pulse-height obtained from anthracene is about twice that obtained using the materials in group 2. The ratio of 2 : 1 for the pulse-heights from anthracene and stilbene is in agreement with the results of Harding *et al.*[7].

(2) The five materials in group 2 are about equally effective. The three materials not previously reported may merit further investigation to obtain information on the effect of rigorous purification, decay time, ease of crystallization, etc.

(3) None of the substances in Group 3 is effective. In particular dibenzyl[1], which has been reported as a useful phosphor, has a very poor performance when it is used with the *EMI* photomultiplier. Koski and Thomas[5] have also found that this substance is poor when used with *RCA* 931*A* type photomultipliers.

(4) All the compounds previously reported are included in the table with the exception of phenyl benzal glyoxalidine[1], terphenyl[4] and diphenylacetylene[5,6]. We have been unable to obtain these substances. The first has been reported as poor, and its omission is not serious. Diphenylacetylene has been reported as good[5,6], and its omission is unfortunate.

We wish to thank Messrs. Petrocarbon, Ltd., for supplying many of the chemicals used in this work.

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Aug. 30.

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Gallium Anisotropy and Crystal Structure

AN earlier communication[1] directed attention to the ease with which single crystals of gallium could be produced and to the highly anisotropic properties of the specimens so obtained. The room temperature electrical conductivities as determined for the three mutually perpendicular axial directions were in the ratios of 1 : 3·2 : 7, and the corresponding expansion coefficients were in the ratios of 1 : 0·7 : 1·9. At that time, the axial directions had been related to the geometrical form and positioning of the seed crystal used when growing the specimens. The seed crystal had the symmetrical pyramidal form which is characteristic of crystals formed from liquid gallium when cooled to some 10° C. below the normal melting point. The axial direction giving the low conductivity had been found to coincide with the geometrical axis passing through the well-defined crystal point, while the other two axial directions coincided with the diagonals of the approximately square base of the pyramid.

An X-ray examination of the gallium seed crystal has now been made by Mr. E. A. Calnan, of the Metallurgy Division of this Laboratory. Back reflexion Laue patterns produced with the beam directed along the pyramid axis of the specimen showed elements of four-fold symmetry, confirming that the crystallographic pseudo-four-fold axis was within about 1° of the geometrical axis of the crystal. This indicates that the *c*-axis is parallel to the pyramid axis. From further consideration of the angles between the plane normals corresponding to the Laue reflexions and the systematic missing spectra, it was possible to identify the *a*- and *b*-axes and to show that their directions coincided with the diagonals of the pyramid base, and thus with the other two conductivity axes.

From subsequent measurements of the electrical conductivity of single-crystal rods grown from this crystal in the identified *a* and *b* axial directions, it has been found that the good conducting direction coincides with that of the *b*-axis. Thus the axial directions designated as 1, 2 and 3 in Table 1 of the earlier communication[1] are now identified as conforming to the *c*-, *a*- and *b*-axes of the gallium crystallographic structure. Thus, it is for the $c : a : b$ axes that the conductivities are in the ratio of 1 : 3·2 : 7, and the expansion coefficients in the ratio of 1 : 0·7 : 1·9.

According to Bradley[2], eight atoms of gallium occupy an orthorhombic cell. The dimensions, obtained by converting from the kX-units originally used, are $a = 4{\cdot}5258$ A., $b = 4{\cdot}5198$ A., $c = 7{\cdot}6602$ A. The space-group is V_h^{18} and the atomic co-ordinates are $(m, 0, p)$ $(m + \frac{1}{2}, \frac{1}{2}, \bar{p})$ $(\bar{m} + \frac{1}{2}, \frac{1}{2}, p)$ $(\bar{m}, 0, \bar{p})$ $(m, \frac{1}{2}, p + \frac{1}{2})$ $(m + \frac{1}{2}, 0, p + \frac{1}{2})$ $(\bar{m} + \frac{1}{2}, 0, p + \frac{1}{2})$ $(\bar{m}, \frac{1}{2}, \bar{p} + \frac{1}{2})$, where $m = 0{\cdot}0785 \pm 0{\cdot}0005$ and $p = 0{\cdot}1525 \pm 0{\cdot}0005$.

It will be seen that the lowest conductivity occurs for the direction having the greatest lattice spacing; but it seems probable that the large (2·2-fold) increase in conductivity between the *a*- and *b*-directions is due to some other cause than a decrease of about 6 parts in 4,500 in the lattice spacing.

There may be some significance in Laves's[3] suggestion that the closest pairs of atoms form Ga_2 molecules. This would reduce the structure to four such molecules per cell, lying symmetrically in the *a*–*c* plane at angles of $\pm$ 17° (approximately) to the *c*-axis, the spacing of these molecular planes being $b/2$. On this

basis, the good conductivity axis would be normal to the molecular plane, while the intermediate and low conductivity axes would be in this plane with the molecules aligned at angles of approximately $\pm 73°$ and $\pm 17°$ to the respective conductivity axes.

The work, which is being continued, forms part of the research programme of the National Physical Laboratory and the above account is published by permission of the Director of the Laboratory.

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July 17.

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Combustion of Fuel Droplets

We were very much interested in Mr. G. A. E. Godsave's letter[1] on the combustion of fuel droplets, for we also have been studying the same subject.

We have measured the time of burning of droplets suspended at the end of a silica filament or metallic wire by a photographic method in still air. The time of burning, T, is found to be directly proportional to the square of the initial diameter of the droplet, d_0, over the range of drop-size investigated (approximately 1·3–0·3 mm.). This is exactly the same as saying that the burning-rate is directly proportional to the diameter, d, of the drop at any instant, which is the result obtained in Godsave's experiment.

We can compare the life of droplets of various kinds of fuel by means of the relation T/d_0^2 (which we call the specific burning-time). When using the platinum wire in our experiment, it was found that the burning-time was considerably shortened. This may be due to catalytic combustion.

We propose to give a detailed account of our work in the *Transactions of the Society of Mechanical Engineers, Japan*.

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Hiroshi Isoda

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Aug. 25.

[1] *Nature*, **164**, 708 (1949).

It is of interest to find that Drs. Kumagai and Isoda's results are in agreement with the earlier measurements at the National Gas Turbine Establishment. The constant T/d_0^2 they refer to as the specific burning-time is the inverse of a quantity referred to in the work at the National Gas Turbine Establishment as the evaporation constant λ. In the case of liquid fuel drops, the latter concept is perhaps of more general significance, as it can be applied to evaporation under both non-burning and burning conditions. In the low-temperature case the evaporation-rate is determined by diffusion processes, and in the high-temperature case the evaporation-rate is determined by heat transfer processes. In both cases experiments have shown that the mass evaporation-rate is proportional to the first power of the radius of the drop. Hence in both cases T/d_0^2 is constant, and may be conveniently referred to in the general case as the specific lifetime of the drop.

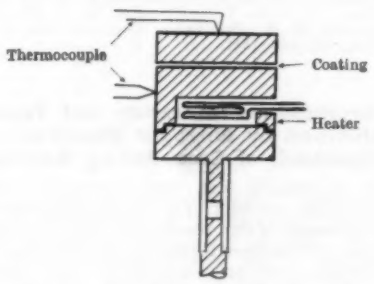
Without further details, it would appear that the suggestion of Kumagai and Isoda that catalytic effects are of importance in the case of suspension on a platinum wire is unlikely. The investigations at the National Gas Turbine Establishment of the mechanism of the combustion of single drops of liquid fuel (in which all the combustion reactions occur in the vapour phase) have emphasized that the heat transfer to the drop is the dominant factor determining its life. This being so, a platinum wire suspension (which presumably through geometric necessity would pass through the flame surrounding the drop) would increase the heat transfer of the drop, and hence would decrease its life as compared to the case of suspension on a silica filament.

G. A. E. Godsave

National Gas Turbine Establishment,
Ministry of Supply.
Nov. 7.

Thermal Conductivity of Barium Strontium Oxide

Only two values of the thermal conductivity of barium strontium oxide have been published: 0·00034 cal. deg.$^{-1}$ cm.$^{-1}$ sec.$^{-1}$ obtained by Clausing and Ludwig[1], and 0·000123–0·0013 cal. deg.$^{-1}$ cm.$^{-1}$ sec.$^{-1}$ by Patai and Tomaschek[2]. Hamaker[3] pointed out, however, that the methods employed by these workers were open to objections, since they depended on optical pyrometric measurements made on coated cathodes, and no allowance was made for the semi-transparency of the coating.



A method based on Lees's disk has been developed to overcome these objections. The assembly, shown in the accompanying diagram, consisted of three nickel disks, 1 in. diameter and $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{1}{4}$ in. wide respectively. The centre $\frac{1}{2}$-in. disk contained an alundum-covered, molybdenum-strip heater closed in by the lower disk. Barium strontium carbonate was sprayed on the top of the centre piece and the top disk pressed on to it. Thermocouples welded to the nickel were used for measuring the temperature-drop across the coating, and leads permitted the passage of current through it for activation purposes. The whole block, supported on a ceramic tube, was erected in a continuously pumped demountable bulb, and the carbonate decomposed by heating the block to 950° C. by a high-frequency coil. After cooling, the heater was switched on, and thermocouple readings taken when a steady state had been reached. Since coatings of the order 50–200 μ were used, the

heat losses at the edges could be neglected and thus the heat conducted through the coating was equated with the heat lost by the top disk. The latter was computed from total emittance values measured as a subsidiary experiment on similar nickel made up into tubes.

If the temperatures of the centre and top disks are T_1 and T_2 respectively, then

$$A_1 l_T \sigma T_2^n = A_2 k(T_1 - T_2)/d,$$

where A_1 is surface area of top disk; A_2 is area of coating; d is thickness of coating; k is thermal conductivity of coating; l_T is total emittance of the nickel; σ is Stefan's radiation constant; n is a constant (~ 4). l_T and n were found by the subsidiary experiment.

Preliminary results have shown values of $1\text{–}5 \times 10^{-4}$ cal. deg.$^{-1}$ cm.$^{-1}$ sec.$^{-1}$ at 1,000° K., increasing slightly with temperature.

The results appear independent of the compression of the original carbonate, and of the state of activation of the oxide.

The low order of these values agrees with those obtained by Aberdeen and Laby[4] for other powders in vacuum, and fits in with the theory of heat conduction in powders proposed by Smoluchowski[5]. Further investigations are in progress.

I wish to thank Dr. J. A. M. van Moll and the directors of Philips Electrical, Ltd., for permission to publish this communication.

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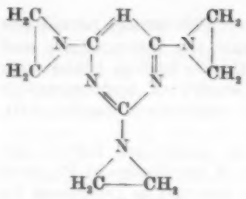
Chromosome Alteration and Tumour Inhibition by Nitrogen Mustards: the Hypothesis of Cross-linking Alkylation

The hypothesis has been advanced that the 'radiomimetic' activity of nitrogen mustards depends on the presence of two reactive groups in the mustard molecule. It has been further suggested that chromosome bridging and breakage, which result from treatment of proliferating cells with mustards, are a direct consequence of interchromatid cross-linkage by single polyalkylating molecules[1,2]. Such a mechanism of chromosomal damage could be considered as a plausible explanation of the effective actions of nitrogen mustards and certain other polyfunctional molecules, such as polyethylenimines and diepoxides, against mammalian tumours. However, recent studies of a number of monofunctional compounds related in structure to nitrogen mustards and diepoxides raise some questions concerning the validity of these generalizations.

Structural alterations in chromosomes resembling those produced by polyfunctional nitrogen mustards can be readily obtained in proliferative cells treated with monofunctional compounds. In one set of experiments, root tips of *Allium cepa* were exposed to solutions of two agents with more than one reactive group per molecule, namely, methyl-*bis*(2-chlorethyl)-amine and 2,4,6-*tris*(ethylenimino)-*s*-triazine (I);



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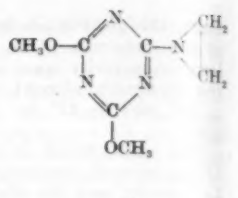

II

they were also treated with various agents with but one alkylating ethylenimino group per molecule, for example, 2-ethylenimino-4,6-dimethoxy-*s*-triazine (II) and ethylenimine itself. The former two agents were used in concentrations of 1·0, 0·1 and 0·01 m*M*/litre. The last two agents were used in the same concentrations and at 10·0 m*M*/litre. Exposure was for one-half hour, after which the bulbs and roots were washed and returned to fresh tap-water. Feulgen squash preparations were made of roots fixed at intervals during four or five days after exposure.

All agents caused chromosome breaks. After methyl-*bis*(2-chlorethyl)-amine, an initial period of mitotic inhibition was followed by the occurrence of mitotic figures showing anaphase bridges and chromosome fragments in highest frequency on the third and fourth days. Most abnormal anaphases were seen in roots treated with 0·1 and 0·01 m*M*/litre of this compound. After (I), abnormal anaphases were noted less frequently than after methyl-*bis*(2-chlorethyl)-amine, and the most effective concentrations were 1·0 and 0·1 m*M*/litre. The monoethylenimine agent (II) caused less mitotic inhibition than the two previous agents but apparently more chromosome breakage. All four concentrations were very effective. Ethylenimine in concentrations of 10·0–0·01 m*M*/litre caused chromosome breaks in considerable frequency; but it was not until the fifth day that they were much in evidence, because a prolonged mitotic inhibition had first to be overcome. Representative abnormal anaphases observed after treatment with these agents are illustrated in Figs. 1–4.

Tissue cultures of mouse embryonic skin and Crocker mouse sarcoma 180 were also used for demonstrating the effectiveness of monofunctional agents in causing structural chromosome changes. Glycidol (2,3-epoxy-1-propanol) and ethylenimine in fractional millimolar concentrations were found, two days after exposure, to elicit anaphasic bridges and chromosome fragments at levels higher than their low normal frequency in the cultured mouse cells.

The capacity of the above monofunctional derivatives to damage proliferating cells is not limited to effects which can be elicited solely *in vitro*. Indeed, compound (II), other monoethylenimine derivatives, and ethylenimine itself, when administered intraperitoneally to rats, have been found to disturb hæmatopoiesis and to give rise to pancytopænia in a manner resembling the well-known effects of mustards. Of great interest has been the observation that aberrant anaphases appear in the bone marrow of rats 72 hr. after administration of sublethal, though leucopænic, doses of (II) and other monoethylenimine derivatives. Myelocytes and metamyelocytes which exhibit the aberrant anaphases contain well-defined chromosome bridges and acentric fragments. In addition to its mustard-like effects in normal hæmatopoietic tissues, compound (II) has been shown to inhibit in mice the growth of transplanted

Cells at anaphase of mitosis from *Allium cepa* root-tip preparations. Acetic-alcohol and Feulgen. (× 1,455)

(1) Anaphase showing one chromosome bridge and two fragments, 92 hr. after a half-hour exposure to 0·1 m*M*/litre 2,4,6-*tris*(ethyleneimino)-*s*-triazine. (2) Anaphase with one dicentric bridge and fragment, 23 hr. after half-hour exposure to 0·1 m*M*/litre 2-ethylenimino-4,6-dimethoxy-*s*-triazine. (3) Anaphase with bridge and small fragment, 72 hr. after half-hour exposure to 0·1 m*M*/litre ethylenimine. (4) Late anaphase with several bridges, 96 hr. after half-hour exposure to 1·0 m*M*/litre ethylenimine; two very small fragments were noted near the bridges

sarcoma 180 and to cause a rapid fall to subnormal levels of the high leukocyte count characteristic of the advanced stage of leukæmia *Ak4*.

It is clear that the monofunctional agents described above cannot elicit 'radiomimetic' effects in chromosomes and proliferating cells by a mechanism involving 'cross-linking' alkylation. Nevertheless, these agents contain reactive groups similar to those present in polyethylenimines such as (I), diepoxides, and the ethylenimonium transformation products of nitrogen mustards. The role of the ethylenimonium moieties in the biological actions of the nitrogen mustards is well known (see review, ref. 3). The 'radiomimetic' effects of diepoxides have recently been described[2], and, in addition, mustard-like actions of polyethylenimines, such as (I), have been reported in normal[4] and tumour-bearing mammals[5–8].

It is recognized that the results reported here do not negate the possibility that the polyfunctional alkylating agents may effect 'radiomimetic' action through a cross-linkage of vital fibrous molecules. Nevertheless, it appears more reasonable to consider the radiation-like activity of the compounds under discussion to be related to the possession of molecular configurations with similar chemical properties, namely, unstable 3-membered heterocyclic radicals. We are not aware at the moment of any molecular class having the 'radiomimetic' activity of nitrogen mustards both *in vitro* and *in vivo*, which does not contain this configuration either in the parent compound or in some active transformation product. Against this tentative generalization may be raised the reported failure to demonstrate ethylenimonium moieties as intermediates in the reactions of the 'radiomimetic' N,N-*bis*(2-chloroalkyl) arylamines[1]. However, it should be noted that N-phenyl-ethylenimine has been prepared synthetically[9]. Thus the existence of N-aryl ethylenimonium transformation products appears possible, although such derivatives of weakly basic aromatic amines might be expected to be unstable and difficult to detect[3].

We wish to acknowledge support of part of the work reported herein by grants from the National Cancer Institute, U.S. Public Health Service, from the American Cancer Society, from the Damon Runyon Memorial Fund, and from the Elsa U. Pardee Fund. We also wish to express our indebtedness to Dr. M. L. Crossley, Dr. R. P. Parker, jun., and their associates in the American Cyanamid Co. for generous supplies of the ethylenimine and ethylenepoxy derivatives used in the present work, and our appreciation to Dr. J. H. Paden of the same Company for information concerning the synthesis of N-phenyl-ethylenimine.

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July 10.

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In the foregoing communication, attention is directed to the ability of certain monofunctional alkylating agents to produce chromosome damage and inhibition of tumour growth. Similar cytological observations have been made in this Institute[1].

Thus, in addition to ethylene imine and ethylene oxide, analogues of the sulphur and nitrogen mustards, namely, ethyl 2-chlorethyl sulphide and 2-chloroethyl dimethylamine, are able to produce chromosome breakage in plant material. However, as has been found in animal experiments by Philips[3], the effect is elicited only by concentrations some fifty times that required of bifunctional analogues. Doses of the same order as used in the case of bifunctional compounds have proved to exert no measurable effect upon the growth-rate of the Walker carcinoma[3]. Ethylene oxide has also been shown by Rapoport[4] and by Bird[6] to be mutagenic for *Drosophila*, and Rapoport has also established the mutagenic activity of ethylene imine[6]. It is therefore apparent that, while monofunctional compounds do produce effects which are similar to those produced by the 'mustards', they are less effective in this respect than difunctional compounds of like chemical character.

In the course of work by our colleague, G. M. Timmis, on other types of polyfunctional compound (which will shortly be published) it was found that 1 : 4-*bis*-methanesulphonoxybutane $CH_3.SO_2.O.(CH_2)_4.O.SO_2.CH_3$ inhibited the growth of sarcoma 180 and produced typical radiomimetic effects in the blood and bone-marrow; also one of us (A. L.) has found that dimethyl sulphate produces chromosome breakage in plant material. Now these compounds cannot form three-membered rings; but they can react by a carbonium ion mechanism, as also can epoxides, ethylene imines and the sulphur and nitrogen mustards. The ability to produce carbonium ions is thus a more general feature of this group of radiomimetic agents than the ability to produce a compound containing an unstable three-membered ring system.

This subject will be dealt with more fully in future publications.

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Identification of the Gene for Maltose Fermentation in *Saccharomyces Italicus*

In 1949, Gilliland[1] established the fact that only one gene for maltose fermentation is present in *Saccharomyces Italicus*. He crossed this species with *S. Chevalieri*, which is unable to ferment maltose and is therefore of the recessive formula *mm*, and obtained a hybrid *Mm*. All the 4-spored asci isolated from this interspecific hybrid yielded two fermenters to two non-fermenters of maltose.

We have found[2,3] that *S. cerevisiæ* (type Yeast Foam) contains three polymeric genes for maltose fermentation, M_1, M_2 and M_3. By crossing *S. cerevisiæ* with the recessive *S. Chevalieri*, we obtained a triple heterozygote, and we succeeded in isolating genotypes possessing M_1 alone, M_2 alone and M_3 alone. In addition, we obtained through spontaneous mutation, as well as mutation induced by Röntgen rays, a fourth *M*-gene which we have called M_4. These four *M*-genes are inherited independently of one another.

By spore-crossing *S. Italicus* with M_1, M_2, M_3 and M_4 yeast types, we have now established that the *M*-gene in *S. Italicus* is M_1. The crossing between an M_1-type and *S. Italicus* yielded a hybrid from which eighty spores were isolated. The subsequent eighty single-spore cultures all fermented maltose. Extensive tetrad analyses were not possible with this material, since the hybrid, like *S. Italicus*, produced almost exclusively 2-spored asci on gypsum.

It was to be expected that each of the hybrids obtained by spore crossings between *S. Italicus* and the M_2, M_3 and M_4 types would be doubly heterozygotic, $M_1m_1M_2m_2$, $M_1m_1M_3m_3$ and $M_1m_1M_4m_4$, respectively. This proved to be the case, as is evident from the following segregation ratios, which likewise were obtained from 2-spored asci:

Crossing	Hybrid No.	Segregation from the hybrids	
		Maltose fermenters	Non-fermenters
S. Italicus × M_2	145	22 = 62·9 per cent	13 = 37·1 per cent
S. Italicus × M_3	147	27 = 67·5 ,,	13 = 32·5 ,,
S. Italicus × M_4	149	26 = 74·3 ,,	9 = 25·7 ,,
		75	35

Theoretically, a 2-gene segregation should give 75 per cent fermenters to 25 per cent non-fermenters, since we had previously demonstrated that there exists a cross-over value of approximately 50 per cent between each of the *M*-genes and its centromere. Thus the 110 single-spore cultures from Hybrids 145, 147 and 149 should have yielded 82·5 fermenters and 27·5 non-fermenters. The observed figures, 75 : 35, differ from the theoretically expected by 7·5, which is only 1·65 times the standard error (4·54).

No fundamental differences in the biochemical nature of the four *M*-genes have as yet been demonstrable.

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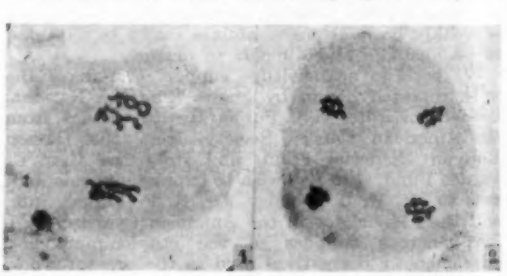
Colchicine-induced Tetraploidy in *Luzula purpurea* Link

Luzula purpurea has been the subject of constant research in our laboratory since Malheiros and Castro[1] observed the unusual behaviour of its chromosomes.

In the spring of 1950, we succeeded in inducing tetraploidy in seedlings of this species, after immersing aerial parts in a 0·5 per cent colchicine solution. Following this treatment, the plants exhibited a striking growth retardment and delay in flowering. One of the treated plants showed the usual morphological characteristics of induced tetraploids, mainly more pointed and thicker leaves with a darker green colour.

Cytological analysis of pollen mother cells revealed chromosome duplication, namely, $2n = 12$ instead of $2n = 6$. Moreover, it was observed that these chromosomes were shorter and thinner compared with the normal ones in diploid plants.

Chromosome behaviour is, in most cases, similar to that described by Malheiros, Castro and Camara[1]. Throughout prophase and metaphase I, they form polyvalent associations, and at anaphase I, 12 chromatids may clearly be seen going to the poles.



Diads are thus formed with 12 daughter chromosomes in each nucleus (Fig. 1). Metaphase II proceeds in the normal way for the species, and after anaphase II, 6 chromosomes are included in each telophasic nucleus (Fig. 2). Every other division until pollen germination seems to follow closely the general features described earlier[1].

In a few cases irregularities were noticed, as, for example, tetrads with 5 and 6 nuclei, containing each a variable number of chromosomes, but never less than 3.

Pollen grains enclosed within the common membrane show a striking swelling, giving the tetrads a very pronounced lobulate appearance.

A more detailed cytological study of these autotetraploids will be published shortly.

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Effect of Azide on the Respiratory System of *Aerobacter aerogenes*

Sodium azide, a substance which has been widely used in the study of various metabolic problems, is known to have three main effects: (*a*) it combines with several metallo-proteins, in particular with hæmatin compounds, a9d thus strongly inhibits respiration[1]; (*b*) it inhibits processes of assimilation and synthesis in micro-organisms[2]; and (*c*) it interferes with the oxidative metabolism of cells by preventing the uptake of inorganic phosphate[3]. The last two effects arise probably from the same cause[4]. The inhibition of respiration is usually very strong and will tend to mask any other effects. Some cases of azide-resistant respiration have been reported[5]; but the phenomenon could not be explained.

For the following experiments a strain of *Aerobacter aerogenes* kindly given to me by Dr. E. F. Gale was cultivated in ordinary broth medium at 30° C. The cells from 16-hr. cultures were thoroughly washed with water and resuspended in 0·1 *M* phosphate buffer. Respiration-rates were measured in Barcroft manometers at 30° C. With glucose as substrate, the respiration of the bacteria was not inhibited by sodium azide, even in concentrations as high as *M*/100 at *p*H's varying between 5·5 and 7·8. In fact, a marked acceleration was observed. However, in presence of succinate or acetate, *M*/300 azide inhibited the oxygen uptake by more than 60 per cent. At first sight one might suppose that the oxidation of glucose does not proceed through the same final chain of reactions as does succinate or acetate. But with these substrates the anaerobic reduction of methylene blue, as measured in Thunberg tubes, was inhibited by azide to the same degree (more than 60 per cent) as was the respiration in manometers. It thus appears that the site of the inhibitory action is a dehydrogenase rather than the final step in the oxidation reaction chain, namely, the cytochromes. Further evidence in favour of this view was obtained in spectroscopic experiments which clearly indicated that azide did not combine with any of the cytochrome components of this organism.

In the oxidation of glucose, *M*/300 azide increased the oxygen uptake by more than 100 per cent and the rate of anaerobic reduction of methylene blue by 300–400 per cent. This action was highly specific for glucose; with fructose, a sugar which was oxidized at the same rate as glucose, or with galactose, azide did not produce any appreciable increase in the rate of oxygen uptake or of anaerobic reduction of methylene blue (Table 1). Moreover, azide had no effect on pyruvate oxidation, a substance which was formed in very large amounts during the oxidation of glucose, and must therefore be an important intermediate in its aerobic breakdown. These facts show that azide probably acts on the earlier steps of glucose oxidation. When washed cells were suspended in glycyl–glycine buffer plus glucose, a similar acceleration by azide was observed.

Table 1. Effect of Azide on the Respiration and on the Anaerobic Reduction of Methylene Blue by *Aerobacter* in the Presence of Various Substrates

Substrate	Q_{O_2}		Methylene blue reduction time (sec.)	
	Without azide	With *M*/300 azide	Without azide	With *M*/300 azide
Glucose	117	245	170	50
Fructose	119	116	165	145
Galactose	51	54	190	170

Table 2. Effect of Azide on the Aerobic Breakdown of Glucose by *Aerobacter aerogenes* at 30°.
6·5 mgm. bacteria (dry wt.). Duration of experiments, 30 min. Results expressed in μmoles

	Without azide	With *M*/300 azide
Oxygen uptake	8·25	17
Glucose utilized	4·06	8·06
Pyruvic acid found	4·7	6·7
Lactic acid found	1·0	0·8
Carbon dioxide output	6·25	14·5
Respiratory quotient	0·76	0·85

A preliminary study of the action of azide on the intermediary metabolism of glucose was made in the following way. Oxygen uptake and carbon dioxide output were measured by the Dixon–Keilin method[6] and the amount of glucose utilized during the experiment was estimated, as were also the pyruvic and lactic acids present at the end of the experiment (Table 2). The quantities of oxygen taken up, carbon dioxide produced and glucose utilized were all doubled by azide; the quantity of pyruvic acid formed was increased by 50 per cent, whereas that of lactic acid was slightly decreased. It is interesting to note that the quantity of pyruvic acid was exceptionally high, even in absence of azide, whereas in anaerobic experiments only very small amounts were found. The carbon deficit, especially in the presence of azide, is probably due to the formation of acetate, for which no analytical figures are available.

Further experiments will show whether the specific action of azide on the oxidative breakdown of glucose is related to phosphorylation.

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Physiological Effects of Trihydroxy-N-Methylindol and its Reaction with Copper Ions

TRIHYDROXY-N-METHYLINDOL, a highly fluorescent substance derived from adrenochrome[1-4], sensitizes the smooth muscles of the cat to the excitatory (nictitating membrane) and inhibitory (non-pregnant uterus) actions of adrenaline[5].

This substance has a great affinity for copper ions. The fluorescence of a 2×10^{-6} solution disappears in three minutes at laboratory temperature in the presence of 1×10^{-6} copper ions (as copper sulphate); it inhibits the oxidation by copper ions of adrenaline in aqueous solution. A substance has been isolated and analysed which corresponds to the formula $C_9H_7NO_3Cu + H_2O$ or $C_{18}H_{14}N_2O_6Cu_2 + 2\ H_2O$ (found: C, 42; H, 3·6; N, 5·1; $C_{18}H_{14}N_2O_6Cu_2$ requires C, 41·5; H, 3·5; N, 5·4 per cent).

The hypothesis may be put forward that the physiological actions of trihydroxy-N-methylindol (decrease of bleeding time[6] and of capillary permeability, sensitization to adrenaline) are related to its copper-binding activity in accordance with the general concept formulated by Lavollay[7], Javillier and Lavollay[8], Clark and Geissman[9], in order to explain the 'vitamin P'-like actions of flavonoids and related substances.

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'Y-Protein': a New Component of the Muscle Machine

WE have recently shown[1] that if rabbit muscle is extracted with a solution of *high* ionic strength (potassium chloride > 0·5), the electrophoretic test (Tiselius–Longsworth, see ref. 2) shows, besides the proteins already described[2-7], a new component that we have called '*Y*-protein'. At ionic strength 0·40 and *p*H 7·4, it moves more slowly than β-myosin.

This new protein exists only in extracts of normal resting muscle. If the muscle has been previously subjected to contracture by pharmacological means (monoiodoacetate) or *rigor mortis*, neither *Y*-protein nor β-myosin can be found in the extracts if potassium chloride is used for the extraction[8]. This is due to the presence of strong linkages between those proteins and muscle stroma, in the contracted state. These linkages cannot be broken by potassium chloride, but are broken by potassium iodide or pyrophosphate solutions[1].

We have succeeded in isolating *Y*-protein. Normal resting muscle is progressively frozen in a − 20° C. chamber, cut in 50-μ slides by use of a freezing-microtome[9], extracted for ten minutes with 5 vol. Weber–Edsall solution (0·6 *M* potassium chloride, 0·04 *M* sodium bicarbonate, 0·01 *M* sodium carbonate; the addition of 0·3 per cent adenosine-triphosphate does not affect the amount of *Y*-protein extracted). Extraction and the following operations are carried out in a cold room at ± 1° C. After centrifugation, the liquid is progressively mixed with saturated ammonium sulphate solution at *p*H 6·00 (53 c.c. sulphate solution for 100 c.c. extract, which corresponds to 35 per cent saturation).

The electrophoretic analysis of the precipitate shows that it contains α-myosin (actomyosin), *Y*-protein and some myogenes. β-myosin remains in the solution (it is precipitated only at 37–40 per cent saturation in ammonium sulphate at *p*H 6·00 [8]). The precipitate containing *Y*-protein is washed with ammonium sulphate solution at 35 per cent saturation and dissolved in 0·1 *M* potassium chloride at *p*H 7·6. The solution, which is rather turbid owing to the presence of actomyosin, is dialysed against a solution of μ 0·05 (0·016 disodium hydrogen phosphate, 0·002 sodium dihydrogen phosphate). The actomyosin is precipitated and *Y*-protein remains in the solution.

Electrophoretic controls show that only one component is present. Its velocity is:

at μ 0·40, *p*H 7·40: $-\ 2{\cdot}7 \times 10^{-5}$ cm./volt/sec.
at μ 0·10, *p*H 7·60: $-\ 3{\cdot}4 \times 10^{-5}$ cm./volt/sec.

In an extract of whole muscle, in which *Y*-protein is present as well as all the other muscle proteins, the velocity of the *Y*-protein is much reduced:

at μ 0·40 and *p*H 7·40: $2{\cdot}65 \pm 0{\cdot}1 \times 10^{-5}$ cm./volt/sec.

Solutions of *Y*-protein are rather clear, slightly viscous and do not show double refraction of flow. Optical properties do not depend on ionic strength between 0·005 and 0·5.

Y-protein is insoluble in distilled water, fairly soluble in solutions at μ 0·005, *p*H 7·4. The fact that the new protein can be extracted only by using solutions of at least a hundred times ionic strength indicates that *Y*-protein is not free in the muscle cell, but connected to some part of the insoluble stroma from which it is liberated by solutions of high ionic strength.

This property explains why *Y*-protein has not been isolated before. It cannot be present in low ionic strength extracts such as those prepared for isolation of the proteins of the myogen group or the *X*-globulins. When extracts of high ionic strength are made, they are usually prepared to extract the myosins—which are precipitated by dilution—and *Y*-protein is not precipitated in those conditions, being still soluble at μ 0·005 *M*.

That the binding forces which maintain the *Y*-protein in the muscle cell are considerably increased in the contracted state seems to be a proof that the new protein takes part in the constitution of the muscle machine.

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Folic Acid, Vitamin B_{12} and Nucleic Acid Synthesis in *Lactobacillus casei*

A RELATIONSHIP between folic acid and certain constituents of nucleic acids in micro-organisms has been known for some time[1]. This has been ascertained more directly by the recent demonstration[2] that partial deficiency of folic acid in a medium otherwise favourable for rapid growth of *Lactobacillus casei* results in impairment of the synthesis of deoxyribonucleic acid but not of ribonucleic acid. According to Stokes[3], folic acid may function in an enzyme system responsible for the synthesis of thymine, which in turn is used for forming nucleic acids. Presumably, this takes place through its deoxyriboside thymidine with involvement of vitamin B_{12}[4], the mediation of which in nucleic acid synthesis has also been shown[5]. This and other functional relationships between folic acid and vitamin B_{12}[6] suggested an investigation of their effects, singly and together, on nucleic acid metabolism.

With *L. casei* (*ATCC* 7469) grown in a medium[2] varied with respect to concentrations of folic acid (Lederle) and vitamin B_{12} (crystalline, Merck), some results of a typical experiment are set forth in the accompanying table. Nucleic acids were extracted by Schneider's procedure[7]. Ribonucleic acid was determined by the orcinol method[8] and deoxyribonucleic acid by the diphenylamine reaction[9] in suitable aliquots of this extract.

Supplement	Ribonucleic acid* (per cent)	Deoxyribonucleic acid* (per cent)	Cell mass* (gm. per lit.)	0·1 *N* acid† per 10 c.c. medium (c.c)
Folic acid 5 μgm./100 c.c.	9·09	1·67	0·21	2·5
,, ,, 200 ,, ,,	8·42	2·82	0·48	12·14
Vitamin B_{12} 5 ,, ,,	9·39	1·25	0·21	2·26
,, 200 ,, ,,	9·50	2·93	0·24	7·50
Folic acid + vitamin B_{12} 5 μgm. each/100 c.c.	10·26	2·26	0·20	2·38
,, ,, ,, 20 ,, ,,	9·74	2·50	0·30	4·76
,, ,, ,, 200 ,, ,,	9·28	3·82	0·68	12·43

* 24-hr. growth; † 72-hr. growth

As may be expected, folic acid enhances the synthesis of deoxyribonucleic acid. Similar results are obtained with vitamin B_{12} although, from growth and acid production, it is seen that it could not completely replace folic acid as a growth factor for *L. casei*. The interesting observation, however, is that there is a pronounced, and more or less all-round additive, effect of folic acid and vitamin B_{12}.

Other related metabolic activities are being studied.

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Methionine and the Formation of Pigment by Yeasts

YEAST 47 [1] grows well in a defined salts–glucose–ammonia medium, containing *p*-aminobenzoic acid, biotin, pantothenic acid and inositol[2]. This medium, supplemented with 500 μgm. of L- (or 1,000 μgm. of DL-) methionine per ml., but containing suboptimal concentrations (0·003–0·04 mμgm./ml.) of D-biotin, yields yeast crops after 72–96 hr. at 28° which are unusual in that they are pink in colour. Other conditions being suitable, the colour is not formed (1) if any other amino-acid is substituted for methionine; (2) if the concentration of D-biotin is raised to 0·08 mμgm./ml.; (3) in the presence of optimal biotin, but suboptimal pantothenate, *p*-aminobenzoic acid or inositol. Formation of the pink pigment is thus associated with the presence of methionine and with a relative deficiency of biotin.

If, however, *p*-aminobenzoic acid is omitted from the medium and replaced by adenine, methionine and histidine[1,2], no pigment is formed; nor is it possible to induce pigment formation in a strain of *Schizosaccharomyces octosporus* examined in these laboratories[3] which requires adenine, methionine and histidine for growth. Again, under conditions otherwise suitable, but in the presence of 25 μgm. of adenine hydrochloride per ml. of medium, pigment formation by yeast 47 is almost, if not entirely, suppressed. These observations, which suggest that a precursor of adenine is a further factor in pigment formation, are possibly related to those of Lindegren and Lindegren[4], who associate the formation of a pink pigment by an artificially induced mutant of *Saccharomyces cerevisiæ* with the interaction of excess methionine with a precursor of adenine (formed as the result of a 'genetic block' in adenine synthesis) and with other substances.

Pigment formation by yeasts under the conditions described is not confined to yeast 47, having been induced in strains of *Schizosaccharomyces pombe*, *Saccharomycodes ludwigii*, *Saccharomyces carlsbergensis* and five single-cell strains of brewer's yeasts (*S. cerevisiæ*). Certain of these strains form pigment in absence of exogenous methionine, possibly because relatively large amounts of methionine are synthesized by the cell.

It is also noteworthy that, wherever the yeast crops are pink, a diazotizable amine (not *p*-aminobenzoic acid) is present in the fermented medium. The amine is not present when the nutritional environment is such that normal, creamy-white yeast crops are obtained.

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Use of Antisera to identify Nodules produced by the Inoculation of Legumes in the Field

In field trials with leguminous crops 'inoculated' with nodule bacteria (*Rhizobium*), it is sometimes important to know what percentage of the nodules that may develop are, in fact, produced by the culture used for inoculation. This problem arose in some work the object of which was to replace ineffective strains of *Rhizobium trifolii*, which are abundant in some British soils, with a more effective strain used for inoculating clover. It was first important to discover whether the inoculant strain could be established in the nodules of the crop under field conditions in which it would have to compete with the native population of *Rhizobium trifolii* already present in the soil. In inoculation trials on such soils, a proportion of the nodules formed in the inoculated plots will be produced by native *Rhizobium* of which a considerable variety of strains may be present. Isolations can readily be made from nodules taken at random from the crop roots; but the identity of these isolates with the strain used for inoculation is difficult or even impossible to determine by cultural methods, because of the similarity between strains of *Rhizobium* in their appearance and behaviour on laboratory media.

Table 1

Locality	Soil type	Experimenter
Bosley Brook, Macclesfield, Cheshire	Peat	E. C. E. Hemsted
Macaulay Institute of Soil Research, Aberdeen	Loam	A. B. Stewart
Woburn Experimental Station, Bedfordshire	Sandy	H. H. Mann
Essex Institute of Agriculture, Writtle, Essex	Clay	B. H. Harvey

The antigenic constitution of *Rhizobium trifolii* (and probably of other groups of *Rhizobium*) is of such a type as to suggest the use of serological tests for this purpose. Agglutination reactions have not shown the presence of any antigens common to all strains but have revealed a number of somatic and flagellar antigens, any of which is found in some strains but not in others; some of these have been found in very few strains. Tests with antisera against six *Rhizobium* strains were made on 161 strains of *Rhizobium trifolii* from soils taken over a wide area in Great Britain. Only seven of these were found capable of agglutination by antiserum against strain *P* 11 and only ten by antiserum against strain 205. It thus seemed possible that the presence of the inoculant strain in the nodules of the crop could be determined by simple agglutination tests on isolates from these nodules, if the strain chosen for inoculation contained one of the uncommon antigens likely to be absent or rare in the local population of *Rhizobium trifolii*. Its scarcity among these local strains could be checked by testing isolates from uninoculated plots.

To test the feasibility of this idea, small field experiments with Montgomery red clover were laid down and sown in September 1943 at the localities shown in Table 1.

Plots were sown with uninoculated seed, others with seed inoculated with *Rhizobium* strain *Cl F*, which is agglutinated with antiserum against strain *P* 11, and others with seed inoculated with strain 205. Both these strains produce nodules effective in fixing nitrogen. In the Macaulay experiment triplicate, and at Bosley Brook and Woburn duplicate, plots two yards square were laid down, the treatment being disposed at random. At Writtle larger single inoculated plots 100 ft. × 50 ft. were separated and flanked by uninoculated plots. Inoculations were carried out with suspensions in skim milk of bacteria from agar slopes.

After two to three months growth, clover plants, taken at random from the plots, were lifted, and isolations of *Rhizobium* were made from nodules also taken at random. A second sampling of plants from the Macaulay experiment was made in the early summer of 1944. The isolates from plots inoculated with strain *Cl F* were tested with antiserum against strain *P* 11, those from plots inoculated with strain 205 with homologous antiserum against this strain, and those from the uninoculated plots with both antisera. The serological technique used was as described by Kleczkowski and Thornton[1]. Table 2 shows the total numbers of isolates tested from each plot in the four experiments and the numbers that gave 0-type agglutination with the test antisera.

Taking all the experiments together, more than half the nodules from plots inoculated with strain *Cl F* contained bacteria that were agglutinated with serum against *P* 11, and only one of the ninety-two isolates from uninoculated plots reacted with this antiserum. Strain 205 was much less successful in infecting the crop. Bacteria that reacted with antiserum against 205 were found in only fourteen of the ninety-two nodules taken from plots inoculated with this strain. But here also serological identification of the inoculum in these nodules was supported by the fact that only two of the isolates from uninoculated plots reacted with 205 antiserum.

These experiments thus suggest the feasibility of using specific antigens as tracers to follow the

Table 2. Tests of *Rhizobium* Isolates from Inoculated Clover: Results from Individual Plots

Locality	Plots inoculated with strain *Cl F*		Plots inoculated with strain 205		Uninoculated plots		
	Total nodules tested	Positive to *P* 11 antiserum	Total nodules tested	Positive to 205 antiserum	Total nodules tested	Positive to *P* 11 antiserum	Positive to 205 antiserum
Macaulay First sampling	6	3	6	1	6	0	0
	6	4	6	0	6	0	0
	6	3	6	1	6	0	0
Macaulay Second sampling	6	3	6	0	6	0	0
	6	4	6	1	6	0	0
	6	3	6	2	6	1	1
Bosley Brook	10	9	10	4	10	0	1
	10	9	10	3	10	0	0
Woburn	9	5	9	1	9	0	0
	9	3	9	0	9	0	0
Writtle	18	5	18	1	18	0	0
Total, all localities	92	51	92	14	92	1	2

behaviour of an inoculant strain of *Rhizobium* in infecting the crop in the field. Identification can be made more certain by the use of more than one test antiserum that reacts with the inoculant strain and by including specific *H*-type reactions. By such means inoculant strains containing specific combinations of less rare antigens should be identifiable. A development of the technique suggested here has been used successfully in more recent experiments carried out in this Department, the results of which will be published elsewhere.

H. G. THORNTON
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[1] *J. Bact.*, **48**, No. 6, 661 (1944).

Effect of Molybdenum Status on the Ascorbic Acid Content of Plants in Sand Culture

THE role of molybdenum in higher plants is generally thought to be closely connected with the assimilation of nitrate nitrogen[1-3]. Requirements appear to be much reduced with ammonium nitrogen[3]; but recent work here has shown that presence of ammonium and nitrate together does not eliminate the need. The nature of the system involved in nitrate assimilation and the step activated by molybdenum are still obscure. The suggestion that ascorbic acid may function in plants as an agent for nitrate reduction is not new[3], but has been given little consideration. Recently, Virtanen and his associates[4,5] have found that seedlings given nitrate nitrogen may be unable to grow if the cotyledons or endosperm are removed unless ascorbic acid or glutathione or other reducing agents are supplied, whereas excised seedlings given ammonium nitrogen continue normal development in the absence of any supplements. These observations were attributed to the *in vivo* effect of ascorbic acid in the seed reserves on nitrate reduction. No information was available, however, as to the molybdenum status of their cultures. There are also reports which indicate that plants supplied with nitrate contain less ascorbic acid than those grown with ammonium nitrogen[6-8]. Other nutritional effects so far reported, except possibly that of manganese[9], seem to have comparatively little or no consistent influence.

In the course of a general investigation of possible means of nitrate or nitrite reduction in plants, it seemed of interest to determine whether molybdenum status might influence the ascorbic acid content of plants under controlled sand culture conditions. Preliminary results given in the accompanying table indicate that molybdenum deficiency consistently causes a striking (and generally significant) reduction in the apparent ascorbic acid content of several crops (as determined by rapid titration of leaf extracts in 5 per cent metaphosphoric acid in copper-free water with 2 : 6 chlorophenol-indophenol). Molybdenum-deficient plants were also injected through the petioles with 2–400 gm. molybdenum in 12–24 hr., while other plants in the *same containers* remained uninjected. There was a detectable rise in ascorbic acid content in 24 hr.; this increased to double that of the non-injected plants in 3–5 days and attained 75–95 per cent of normal (molybdenum-supplied) plants. These effects were significant. The first visual response to molybdenum was just perceptible in three days as chlorophyll formation, and growth-response could be detected about seven days after injection. No further increase in ascorbic acid occurred when molybdenum was supplied to normal plants in excess of the optimum requirement. Molybdenum-deficient cauliflower plants given ascorbic acid in the nutrient showed fresh chlorophyll formation after about seven days, followed by renewed growth. Addition of ascorbic acid to molybdenum-deficient cultures of *Aspergillus niger* produced a similar growth response, and normal cultures had a higher reducing activity. The ascorbic acid content of cauliflower plants, kindly provided by Dr. W. Plant from a field-trial on an acid soil (Lower Greensand, *p*H 5·5), and showing typical symptoms of molybdenum deficiency[1], was also significantly less than in normal plants which had been treated with molybdenum.

Crop	Ascorbic acid (mgm./100 gm.) (mean values)		− Mo as % of + Mo
	− Mo	+ Mo	
Tomato	56·1	121·1	45·2
Brussels sprouts	48·9	155·3	31·4
Marrowstem kale	58·2	169·9	34·2
Cauliflower	65·7	263·9	24·9
Rape	55·2	190·7	28·0
Savoy cabbage	68·5	184·5	37·1
Alsike clover	92·5	209·0	44·2
Barley	56·5	139·5	40·5
Sugar beet	25·6	75·4	33·9

It would seem that neither disparity in growth nor lack of chlorophyll *per se* accounts for the effect of molybdenum. Any assumption that molybdenum *directly* activates a reaction between ascorbic acid and nitrate or nitrite would require the expectation of *reverse* results from those reported, and in any event ascorbic acid can reduce nitrite and is decomposed slowly also by nitrate non-enzymatically and without metallic activation at 30–35° C.[10]. The failure to obtain further response to injection of additional molybdenum into normal plants does not exclude the possible role of this element in ascorbic acid synthesis, as lack of precursors might have limited further production.

Two other possibilities, however, at present equally compatible with the results obtained, merit consideration. Crook and Morgan[11] found that the reduction of dehydroascorbic acid by glutathione, although appreciable chemically, was greatly accelerated in many plant extracts by an enzymic system probably distinct from the systems responsible for ascorbic acid oxidation, and suggested this mechanism was responsible for the 'protective action' of sulphydryl groups. Waygood[12] and Davison[13] suggested that ascorbic acid is cyclically oxidized and reduced in systems linked to malic dehydrogenase in wheat and formic dehydrogenase in pea. The possibility of linkage between nitrate reduction in higher plants and an anaerobic dehydrogenase via the catalytic influence of ascorbic acid (and dehydroascorbic acid) and its 'protective' intermediates[11,12] seems feasible. Activation of either the dehydrogenase or the dehydroascorbic reductase by molybdenum might account for the results obtained here.

The alternative to be considered at present is that molybdenum may activate some other system responsible for rapid nitrate reduction. When this system fails to operate, the accumulated nitrite[1,2] may then cause oxidative loss of ascorbic acid extra to that due to other known systems. The disturb-

ance of the normal ascorbic acid equilibrium in the plant might contribute to the characteristic symptoms caused by molybdenum deficiency. These views appear consistent with the requirements for molybdenum in the presence of different nitrogen supplies observed described above.

It is hoped to test, and distinguish between, these possibilities by investigations now in progress and to be reported later elsewhere.

Acknowledgment is made to the Agricultural Research Council for providing special apparatus for work on molybdenum deficiency. One of us (E. W. J.) is also indebted to the Council, and another (S. C. A.) to the U.P. Government, India, for providing grants which have enabled them to participate in the work. We also wish to acknowledge the advice of Dr. A. Pollard on the estimation of ascorbic acid in plant extracts.

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Long Ashton Research Station,
University of Bristol. July 29.

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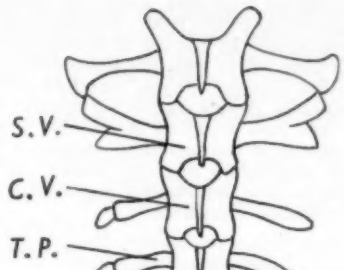
Caudal Ribs in Geckos

During a comparative study of the osteology of some Egyptian geckos, including *Stenodactylus stenodactylus*, *Tropiocolotes tripolitanus*, *T. steudneri*, *Ptyodactylus hasselquistii*, *Hemidactylus turcica*, *Tarentola mauritanica* and *T. annularis*, a rather interesting detail concerning the caudal region of the vertebral column was observed, which has not previously been recorded in living Lacertilia, so far as we are aware. It concerns the presence of caudal ribs in geckos. Among the above-mentioned species, such ribs were found in both *Tarentola annularis* and *Ptyodactylus hasselquistii*.

As a matter of fact, the caudal vertebræ of Lacertilia are provided with prominent transverse processes, which are particularly well developed in some lizards such as *Uromastyx ægyptia*[1]. It was found that in both *Tarentola annularis* and *Ptyodactylus hasselquistii* the transverse processes of the anterior caudal vertebræ carried at their distal ends small pieces of bone representing caudal ribs. These ribs are clearly articulating with the transverse processes, as seen in skeletons stained with alizarine and cleared according to Hollister's method[2]. They are present on one side of the body only (see accompanying illustration). On the opposite side, the lateral process of each rib-bearing vertebra is equal in length to both the transverse process and caudal rib of the same vertebra. It is therefore reasonably assumed



Tarentola annularis. Dorsal view of the two sacral vertebræ and the anterior five caudal vertebræ. *C.R.*, caudal rib; *C.V.*, caudal vertebra; *S.V.*, sacral vertebra; *T.P.*, transverse process

that the caudal ribs, which are freely articulating with the transverse processes of one side, are completely fused with the corresponding processes of the opposite side.

In primitive Tetrapoda ribs are present in the different parts of the vertebral column, including not only the cervical, dorsal and sacral, but also the caudal region. Thus Williston[3] states that in Amphibia and reptiles the ribs "primitively articulate with all vertebræ, at least as far back as the middle of the tail". In the fossil amphibian *Eogyrinus* the anterior caudal vertebræ are provided with rib facets, as described by Watson[4]. Long and massive caudal ribs, which are quite similar to the dorsals, have been also described by Watson[5] in the fossil reptile, *Seymouria*. Thus the well-developed caudal ribs described in fossil Tetrapoda are still retained in the form of a series of rudimentary structures present in the caudal region of some living geckos.

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p-Hydroxybenzoic Acid: a New Bacterial Vitamin

Mutant 83-1 of *Escherichia coli*, isolated by the penicillin method[1], is blocked at an early stage in aromatic synthesis; it responds to a quadruple supplement of tyrosine, phenylalanine, tryptophane and *p*-aminobenzoic acid. Whereas the quantitative requirements for the three amino-acids are similar to those of mutants with single requirements for these compounds, growth is very slow with 0·005 μgm./ml. of *p*-aminobenzoic acid, a concentration sufficient for optimal growth of a mutant blocked

specifically in *p*-aminobenzoic acid synthesis. Higher concentrations of the acid, up to 1 μgm./ml., provide more rapid, yet less than normal, growth of mutant 83-1. Normal growth-rate, however, appears with the addition of shikimic acid (a 2,3,4,5-tetrahydrogallic acid).

Shikimic acid has been reported to be a precursor of the above four aromatic metabolites[2,3]. The facts presented here lead one to postulate a fifth metabolite, compound *Y*, that can be formed readily from shikimic acid and less readily from *p*-aminobenzoic acid. A substance with *Y* activity was, indeed, found in culture filtrates of the parent wild type, and was distinguished by paper chromatography (using response of 83-1 for recognition) from *p*-aminobenzoic acid and shikimic acid. Chromatography was complicated by the presence of *Y* activity in the paper (Whatman No. 1).

The derivation of *Y* from shikimic acid or *p*-aminobenzoic acid in the cell suggested the possibility of a simple aromatic structure. The activity in filter paper further suggested that the substance involved might already be known from studies on wood chemistry. Fifty available aromatic compounds were therefore tested. Among these, only *p*-hydroxybenzoic acid was active; as little as 0·01 μgm./ml. supported rapid growth of 83-1 in the presence of its quadruple supplement. The active materials in culture filtrate and filter paper were indistinguishable from *p*-hydroxybenzoic acid by paper chromatography with three solvents. The high order of activity shown by *p*-hydroxybenzoic acid leads us to propose that it be considered a bacterial vitamin. It may be further noted that wild type *E. coli* also excretes several other vitamins, including *p*-aminobenzoic acid[3].

The method of mutants has been widely used for the discovery of metabolic precursors; the present observations represent an extension to the recognition of a new metabolite. We have found *p*-hydroxybenzoic acid, like *p*-aminobenzoic acid, in significant concentrations (relative to microbiological requirements) in yeast autolysate but not in liver extract. *p*-Hydroxybenzoic acid, therefore, has promise as a model for synthesis of chemotherapeutic analogues selectively toxic to micro-organisms. This hope is encouraged by the observation that the growth-slowing effect exerted on *E. coli* by 100 μgm./ml. of 4,4′-dihydroxydiphenyl sulphone (kindly furnished by Dr. M. E. Hultquist of the American Cyanamid Company) is partly overcome by 0·1 μgm./ml. of *p*-hydroxybenzoic acid.

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[1] Davis, B. D., *Proc. U.S. Nat. Acad. Sci.*, **35**, 1 (1949).
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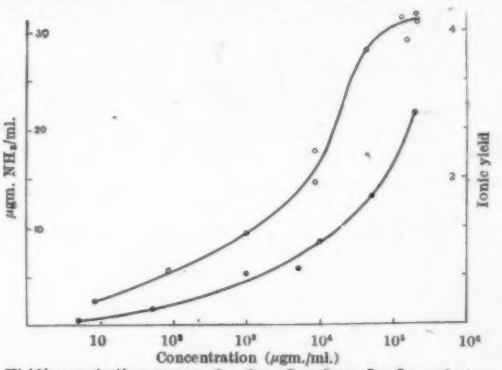
Deamination of Aqueous Solutions of L-Serine by X-Radiation

THE deamination of glycine by X-radiation[1] is dependent on the concentration of the glycine solution in a way that is different from the more common relationship of yield/concentration found for solutions of many inorganic solutes[2] and viruses[3], and, for example, carboxypeptidase[4]. The inactivation of this enzyme by X-radiation is independent of the concentration over a wide range, except in extremely dilute solutions (less than 3×10^{-6} *M*). The yield of ammonia, however, from glycine and other amino-acids shows a continuous rise up to nearly saturated solutions, that is, an approximately 20 per cent solution in the case of glycine. The liberation of sulphur from thiourea[5] follows a similar course. The yield/concentration relationship is not confined to substances of low molecular weight, for McDonald has recently found[6] that trypsin, which has apparently the same molecular weight as carboxypeptidase, also shows a similar rise up to a 1 per cent solution (the highest concentration which could be used for technical reasons).

It was of interest to find conditions under which it would be possible to show that the rise of yield with concentration of the amino-acid, which cannot be expected to continue indefinitely, would show signs of a 'levelling off'. Glycine, while possessing a reasonably high solubility, has too low an ionic yield for this purpose. On the other hand, DL-serine, which has the advantage of an ionic yield almost double that of glycine at equimolar concentrations (ionic yield for DL-serine is 4·2 for a 5 per cent or nearly saturated solution), is far too insoluble.



Yield/concentration curve: ○—○—, L-serine; ●—●—, glycine. X-ray dose, 166,000 r.

L-Serine, however, combines the two features—high solubility and high enough ionic yield—desirable for a study of this kind. The accompanying graph compares the deamination of glycine and L-serine over the complete range of concentrations and shows that, in general, the curves for both acids follow similar trends. In the L-serine curve, however, at nearly saturated solution there is a distinct 'levelling off' tendency in the yield of ammonia. All solutions in this investigation were air-saturated, and the concentrations stated are weight of amino-acid/ml. of solution.

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FORTHCOMING EVENTS

*(Meeting marked with an asterisk * is open to the public)*

Tuesday, January 2—Friday, January 5

INTERNATIONAL ASSOCIATION FOR HYDRAULIC RESEARCH (at Bombay, India).

Tuesday, January 2—Saturday, January 6

BRITISH TRUST FOR ORNITHOLOGY (at the Edward Grey Institute, Oxford).—Annual Conference on "Bird Biology for Students".

Wednesday, January 3

INSTITUTE OF FUEL (at the James Watt Institute, Great Charles Street, Birmingham), at 2.30 p.m.—Papers on "Drying of Fuel, Sand, etc.".*

MANCHESTER FEDERATION OF SCIENTIFIC SOCIETIES (in the Chemistry Lecture Theatre, The University, Manchester), at 3 p.m.—Prof. H. Graham Cannon, F.R.S.: "Animals that Live in the Sea" (Christmas Lecture for Young People). (To be repeated on Thursday, January 4, at the same time and place.)

ROYAL METEOROLOGICAL SOCIETY (at 49 Cromwell Road, London, S.W.7), at 5 p.m.—Mr. F. H. Ludlam: "Clouds". (Vacation Meeting.)

ROYAL PHOTOGRAPHIC SOCIETY, KINEMATOGRAPH SECTION (joint meeting with B.K.S. at Film House, Wardour Street, London, W.C.2), at 7 p.m.—Mr. Brian Langley: "The Split Beam Process".

Thursday, January 4

INSTITUTE OF METALS, BIRMINGHAM SECTION (at the James Watt Memorial Institute, Great Charles Street, Birmingham), at 6.30 p.m.—Discussion on "Temperature Measurements".

BRITISH INSTITUTION OF RADIO ENGINEERS, NORTH-WESTERN SECTION (at the College of Technology, Manchester), at 6.45 p.m.—Mr. G. W. L. Davis: "Marine Radar".

INSTITUTION OF WORKS MANAGERS, BRISTOL BRANCH (at the Royal Hotel, Bristol), at 7.15 p.m.—Mr. A. B. Armstrong: "Motion Study".

Friday, January 5

INSTITUTION OF WORKS MANAGERS, NOTTS AND DERBY BRANCH (at the Midland Hotel, Derby), at 7.30 p.m.—Mr. J. W. Hendry: "Production Efficiency via Time Study".

Saturday, January 6

BRITISH INTERPLANETARY SOCIETY (in the Tudor Room, Caxton Hall, Westminster, London, S.W.1), at 6 p.m.—A Symposium on "The Orbital Rocket".

APPOINTMENTS VACANT

APPLICATIONS are invited for the following appointments on or before the dates mentioned:

HONOURS GRADUATES (with experience in medical or veterinary research, and specialization in experimental pathology, bacteriology, virology, or immunology) at a Ministry of Supply Research Establishment in South-West England—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting G.326/50 (January 6).

LECTURER IN CHEMISTRY—The Clerk to the Governors, South-East Essex Technical College and School of Art, Longbridge Road, Dagenham, Essex (January 6).

TECHNICAL OFFICER (man, with a degree in Electrical Engineering and/or an equivalent professional qualification, and industrial experience in heavy electrical engineering) as Assistant to the Head of the Electrical Engineering section of the Technical and Scientific Register—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting D.381/50A (January 6).

HONOURS GRADUATES (with experience in medical or veterinary research, and specialized in bacteriology, biochemistry, biophysics, immunology, virology or pathology) at a Ministry of Supply Research Establishment in South-West England—The Ministry of Labour and National Service, Technical and Scientific Register (K), York House, Kingsway, London, W.C.2, quoting No. G.339/50A (January 8).

ENGINEER OFFICERS (Mechanical and Aeronautical) and ELECTRICAL OFFICERS (with a previous commission in R.N. or a University degree of B.Sc. (Engineering) or acceptable equivalent) in the Royal Australian Navy—The Royal Australian Naval Liaison Officer, Dept. ENA, Canberra House, 87 Jermyn Street, London, S.W.1 (January 12).

LECTURER IN BOTANY AND GENERAL BIOLOGY—The Principal, Wigan and District Mining and Technical College, Wigan (January 15).

LECTURER or SENIOR LECTURER in the subject of INDUSTRIAL RELATIONS—The Secretary of University Court, The University, Glasgow (January 15).

PHYSICIST—The Secretary to the Management Committee, Liverpool Radium Institute, Liverpool (January 15).

PRINCIPAL SCIENTIFIC OFFICER (with a first- or second-class honours degree, preferably in Naval Architecture, or high professional attainments in that subject) in the Ship Division of the National Physical Laboratory, Teddington—The Secretary, Civil Service Commission, Scientific Branch, Trinidad House, Old Burlington Street, London, W.1, quoting No. 3408 (January 18).

ELECTRICAL ENGINEER or PHYSICIST with knowledge of application of electronics (except radio) to scientific investigation and development work—The National Coal Board, Establishments (Personnel), Hobart House, Grosvenor Place, London, S.W.1 quoting TT/281 (January 24).

PHYSICIST with special knowledge of the properties of matter or of aerodynamics with reference to the problems of flow—The National Coal Board, Establishments (Personnel), Hobart House, Grosvenor Place, London, S.W.1, quoting TT/282 (January 24).

TECHNICAL SECRETARY (with an honours degree in science, with chemistry or fuel technology as major subjects, or an honours degree in technical science, in fuel technology or chemistry, or equivalent qualifications), Coal Research Section, North Ryde, Sydney—The Chief Scientific Liaison Officer, Australian Scientific Liaison Office, Africa House, Kingsway, London, W.C.2, quoting No. 3162 (January 29).

PROFESSOR and a SENIOR LECTURER IN THE DEPARTMENT OF LOCAL GOVERNMENT AND PUBLIC ADMINISTRATION of the University of the Witwatersrand, Johannesburg—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (January 31).

READER IN PATHOLOGY at the University of Adelaide—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (January 31).

SENIOR LECTURER IN THE DEPARTMENT OF PATHOLOGY, University College of the West Indies—The Secretary, Senate Committee on Higher Education in the Colonies, Senate House, University of London, London, W.C.1 (January 31).

LECTURER IN EDUCATIONAL PSYCHOLOGY—The Registrar, University College, Hull (February 10).

JUNIOR LECTURERS IN TROPICAL MEDICINE, PROTOZOOLOGY AND HELMINTHOLOGY—The Dean, London School of Hygiene and Tropical Medicine, Keppel Street, Gower Street, London, W.C.1 (February 28).

PROFESSOR OF PHILOSOPHY at the University of the Witwatersrand, Johannesburg—The Secretary, Association of Universities of the British Commonwealth, 5 Gordon Square, London, W.C.1 (February 28).

BIOCHEMIST (who will also undertake hydrography), and MARINE BIOLOGISTS (2), in the West African Fisheries Research Institute being established in Sierra Leone—The Under-Secretary of State, Colonial Office (Research Department), Sanctuary Buildings, Great Smith Street, London, S.W.1.

ELECTRICAL ENGINEER (hydro-electric) to the Central Electricity Board, Federation of Malaya—The Crown Agents for the Colonies, 4 Millbank, London, S.W.1, quoting M.25728.B.

METEOROLOGIST in the East African Meteorological Department—The Director of Recruitment (Colonial Service), Colonial Office, Sanctuary Buildings, Great Smith Street, London, S.W.1, quoting 27118/10/50.

WORKS ENGINEER in the Division of Atomic Energy (Production), Ministry of Supply Factory, Capenhurst, to be responsible for all factory engineering services—The Ministry of Supply, Division of Atomic Energy (Production), Risley, Warrington, Lancs, quoting Ref. CB/2.

REPORTS and other PUBLICATIONS

(not included in the monthly Books Supplement)

Great Britain and Ireland

Anglo-American Council on Productivity. Productivity Report: Packaging. Pp. xiii+58+11 plates. (London and New York: Anglo-American Council on Productivity, 1950.) 2*s.* 6*d.* [510

Philosophical Transactions of the Royal Society of London. Series A: Mathematical and Physical Sciences. No. 857, Vol. 243: A Theory of the Origin of Microseisms. By M. S. Longuet-Higgins. Pp. 36. 8*s.* No. 858, Vol. 243: Thermodynamics and Rheological Behaviour of Elasto-Viscous Systems under Stress. By F. H. Garner, A. H. Nissan and G. F. Wood. Pp. 37–66. 7*s.* (London: Cambridge University Press, 1950.) [510

Philosophical Transactions of the Royal Society of London. Series B: Biological Sciences. No. 619, Vol. 234: The Transplantation of Normal Tissues, with Special Reference to Auto- and Homotransplants of Thyroid and Spleen in the Anterior Chamber of the Eye, and subcutaneously, in Guinea-pigs. By M. F. A. Woodruff and Hazel G. Woodruff. Pp. 559–582+plates 28–30. (London: Cambridge University Press, 1950.) 11*s.* [510

Other Countries

Proceedings of the American Academy of Arts and Sciences. Vol. 78, No. 3: Some Interspecific Relationships in *Phacelia* subgenus *Cosmanthus*, by Lincoln Constance; On the Theory of Acoustic Radiation Pressure, by J. S. Mendousse; Thermodynamic Relations in a-Variable Systems in Jacobean Form, Part I, General Theory and Application to Unrestricted Systems, by F. H. Crawford. Pp. 135–184. 1.25 dollars. Vol. 78, No. 4: The Lower Permian Insects of Kansas, Part 10, The Order Protorthoptera—The Family Liomopteridæ and its Relations. By Frank M. Carpenter. Pp. 185–218. 1 dollar. (Boston, Mass.: American Academy of Arts and Sciences, 1950.) [510

World Health Organization. Technical Report Series, No. 20: Joint ILO/WHO Committee on the Hygiene of Seafarers: Report on the First Session. Pp. 10. (Genève: World Health Organization; London: H.M. Stationery Office, 1950.) 9*d.*: 10 cents. [510

Acta Argentina de Fisiologia y Fisiopatologia. Vol. 1, No. 1. Pp. 291. (Cordoba: Universidad Nacional, 1950.) [510

Canada: Department of Resources and Development, Development Services Branch, National Museum of Canada, Bulletin No. 115 (Biological Series No. 39): The Flora of Bic and the Gaspé Peninsula Quebec. By H. J. Scoggan. Pp. iv+399 (8 plates.) (Ottawa: King's Printer, 1950.) 1 dollar. [510

Report of the Twenty-seventh Meeting of the Australian and New Zealand Association for the Advancement of Science, Hobart Meeting, January 1949. Edited by Prof. S. Warren Carey. Pp. xl+231. (Hobart: Government Printer, 1950.) [510